American SOCIOLOGICAL Review

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BASIC PROBLEMS OF CONTEMPORARY SOCIOLOGY*

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THE problems which I am about to discuss are basic in the sense that the further development of sociology as a science and the possibility of its effective application to contemporary social problems depend on their solution.

In surveying the studies in which the majority of American sociologists have recently been engaged, we notice certain predominant trends. First, a striving for methodical perfection, although the standards of perfection still differ considerably, if not so much as they did thirty years ago. Second, the selecting of specific problems which can presumably be solved by the factual evidence derived from direct observation of individual behavior or from information about individual attitudes obtained by symbolic communication. Third, the testing of hypothetical solutions by searching for more factual evidence and using it in accordance with the same methodical standards.

Thus, most of the research of American sociologists is carried on within areas where these conditions can be fulfilled. Such an area may be a laboratory, a clinic, a hospital, a place where a small number of people regularly congregate, a kindergarten, a school building with or without its neighborhood, a classroom within a college or a whole college campus, a prison, a summer camp, a military center, a section of a factory or an entire factory, sometimes also the area where the workers live, a village, a town, the habitat of a tribal or rural community, or an ecological part of a city. If the area is relatively large and contains too many people for direct observation and communication, a presumably relevant sample is chosen. Eventually, the same investigator or other investigators carry on the same kind of research in other similar areas.[1]

Undoubtedly, many of these studies have either reached or probably will reach scientifically valid sociological or sociopsychological conclusions. Unfortunately, the significance of these conclusions for the advancement of sociology as a general science is limited. Most of the investigators are concerned only with what is occurring within the relatively narrow field of their research during the time while the research is going on. They ignore the enormous multiplicity and complexity of social phenomena developing on the national scale, the continental scale, and the world-wide scale, as well as the historical background of these phenomena. They seem to be unaware that the most urgent task of sociologists is to investigate these phenomena and find some solutions for the pressing practical problems which they involve.

* Presidential address, delivered at the 1954 annual meeting of the American Sociological Society, at Urbana, Illinois.

[1] This survey is based partly on reports of Research Committees of the American Sociological Society, but mainly on articles published in sociological periodicals. Similar trends, though less general, are noticeable among British and French sociologists.

Of course, a minority of sociologists do include studies of these phenomena in their general sociological theories. As we know, such theories started in the middle of the nineteenth century, and their generalizations extend far beyond the limits of any particular area or any particular country during any particular time. Certainly, most of these theories have proved in some respects methodically defective, and the factual evidence which they use is not always as reliable as that of our local researchers. But is it not better to have defective general theories with insufficient evidence than no theories at all? Their defects can eventually be corrected and their evidence supplemented by new evidence.

I believe that the most serious defect of our systematic sociological theories, a defect which goes back to Auguste Comte, is the antithesis between "social statics" and "social dynamics." Although nowadays relatively few sociologists use the term "statics," they have substituted for it the term "social structure," in contrast with "social change." Many textbooks include a number of chapters about social structures and then separate chapters about social changes. Some books deal almost exclusively with social structure; others exclusively with social change. The term "structure" has been applied to total "societies" on all levels, in the sense in which Comte and his continuators conceived societies, that is, as territorially circumscribed, politically united collectivities; but many sociologists apply it also to rural communities, some to cities, and most of them think that every organized social group has a structure of its own.[2]

What is the implication of the concept of structure? It obviously goes back to the natural sciences. Whatever has a structure must be a system of interdependent components. A complex organism has an anatomical structure. A chemical compound has a structure, inasmuch as it is a system of interconnected elements. Even such a technical product as a house has a structure, composed of interconnected stones or bricks and pieces of wood. Moreover, systems can be classified by their structures. Zoologists and paleontologists have classified animals by their anatomical structures, and chemical compounds are classified by the way in which their elements are interconnected.

How about social systems? Should we assume that every social system, from a large society down to a small family, has a structure because it is composed of human beings as elementary units? This would take us back to the old doctrine that every human collectivity or society must be explained by the biopsychological essence of individual participants. It is the kind of human ontology which underlies most demographic studies, including statistics of human beings and their classification by specific indexes. It is favored by psychologists of certain schools.

However, the majority of contemporary sociological theories are actually based not on studies of men as ultimate entities, but on studies of social interaction between men. And social interaction involves combinations of social actions, i. e., actions of which men are the main objects. If this is so, then social systems must be *systems of social actions*.[3]

[2] Some sociologists, however, opposed the theory that society has a static structure. They introduced instead the conception of "social process" as the main component of society. "Its core is the notion of movement, change, flux . . . of society as a continual becoming" (cf. Max Lerner's article on "Social Process" in the *Encyclopedia of Social Sciences*). This conception was first fully applied by Edward A. Ross in his main work *Principles of Sociology* (New York, 1920), based on a comparative study and classification of social processes. He describes thirty-five different processes. Recently, a different, scientifically more productive concept of social processes has been introduced by Joyce O. Hertzler. He uses it as a heuristic concept leading to the development of a systematic theory as a dynamic whole. Cf. *Society in Action: A Study of Basic Processes,* New York: The Dryden Press, 1954.

[3] These actions obviously differ from actions which do not deal with men, but with other kinds of objects: actions of producing and using technical objects, actions of creating or recreating works of art, actions of religious cultus propitiating deities, actions of problematizing scientific theories, making new discoveries, and formulating new theories, and the like. (Cf. Florian Znaniecki, *Cultural Sciences,* University of Illinois Press, 1952.) There have been many controversies concerning the connection between social actions and these other categories of actions. Compare, e.g., Pitirim A. Sorokin's famous work *Social and Cultural Dynamics,* 4 vols. (American Book Co.: New York,

Now, what kind of empirical evidence are sociologists using when they define and classify social systems (in the sense of systems of social actions) according to their structures? This evidence is derived from the standards of valuation and the norms of active conduct which the combinations of social actions performed by interacting agents are expected to conform. Most of these standards and norms are explicitly formulated, orally in traditions, customs, and mores, or in written documents —legal, political, ethical, and theological. So long as the formulated rules of social interaction remain the same, the social systems to which they apply are supposed to be essentially alike and have a stable axionormative structure.

We cannot deny that verbally formulated standards and norms of social interaction are important empirical sources for sociological studies, especially when we are studying the social systems of a distant past. How little we would know about the social life of ancient Egypt without documentary evidence concerning the standards and norms which regulated its religious, administrative, military, and economic activities! But standards and norms, even when explicitly formulated and accepted by the participants in a collectivity at a certain time, are not sufficient evidence on which to base a consistent, inclusive theory of social systems.

In the first place, if we agree that social systems are systems of interdependent social actions, we should study the actions themselves in order to discover not only what people are supposed to do, but what they are really doing. This is, of course, a much more difficult task. It means that we must investigate social systems from the time they begin to be formed throughout their duration. And since the social systems within a collectivity are not isolated, for the same individual simultaneously or successively participates in several systems, we shall find that the actions included in a particular system undergo various changes in the course of its duration, depending on its dynamic connections with other systems.

In the second place, as we know, the standards and norms which regulate social systems not only differ in human collectivities in various parts of the world, but have been changing everywhere, whether rapidly or slowly. Numerous new species of social systems have evolved in particular collectivities and expanded beyond their limits. Unless we want to ignore entirely the historical past of contemporary collectivities, we must investigate this evolution and expansion.

For many years, with the help of a few assistants and numerous students, I have been trying to develop gradually a dynamic approach to all kinds of social systems. Because social systems and human collectivities in general last only if the agents who participate in them cooperate with one another, I have concentrated on the study of cooperation and considered the study of conflicts, whether within or between systems, as a secondary task.

I distinguish four logical classes of social systems. First, *social relations,* or interpersonal relations, as systems of functionally interconnected actions of two cooperating individuals; second, *social roles,* as systems of functionally integrated cooperative relations between a particular individual and a number of others; third, *social groups,* as functionally integrated systems of social roles which their members perform; fourth, *societies,* as systems of diversified, functionally integrated social groups.

Here I shall omit all reference to social relations,[4] for they have little, if any, direct influence upon those complex, statewide and world-wide social phenomena whose investigation should be the main task of contemporary sociologists. I begin, therefore, with social roles.

I borrowed the term "social role" from Park and Burgess twenty-five years ago and have redefined it gradually in an attempt to make it heuristically useful.[5] I

1932–41) with the recent joint work edited by Talcott Parsons and Edward S. Shils, *Toward a General Theory of Action* (Harvard University Press, 1951). These controversies, however, are being solved by the development of such special branches of sociology as industrial sociology, sociology of art, of religion, of knowledge.

[4] Some results of my studies will be given in a paper in the section on Social Theory under the title "The Dynamics of Social Relations."

[5] I applied it in 1931–33 at Teachers College, Columbia University, in my studies of the in-

shall not take time now to compare social roles with theatrical roles, but merely state that, in my opinion, the similarities underlying their differences justify the use of this common term.

Every social role is performed within a *social circle* of people who accept a particular individual as a *person* presumably fit for the performance of this role. A comparative study of the emergence of particular social roles shows that an individual is accepted and evaluated positively as a person when he has already started to perform or is expected to perform sooner or later definite actions which participants in his circle consider desirable because they will contribute something to the values which the circle shares.

These actions together constitute his *function* as the central person of this social circle. His circle cooperates with him by granting him and actively supporting those *rights* which he needs in order to perform effectively his functions. In short, it is the individual's function, a dynamic combination of his actions, which is the main component of his role as a system. This typically applies to such roles as that of a physician in relations with his patients, a craftsman or merchant in relations with his customers, a minister of religion in relations with his parishioners, and a college student in relations with his instructors and fellow-students.

You are probably wondering why I do not mention "status" in connection with role. I formerly used this term to denote the rights of a person who is performing a specific function. But at least half a dozen different definitions of status have been used during the last eighteen years.[6] To avoid confusion with these various uses, I have dropped the term altogether. In the twelve chapters on social roles in the book I am now writing, I have not used it once, for the simple reason that I did not need to do so.

If we study social roles comparatively in historical perspective, we find a continual evolution of new varieties. This is well illustrated by the evolution in the course of human history of the so-called occupational roles, i.e., roles which include economic remuneration. In tribal societies, more than fifteen specialized occupations are seldom found. In the United States in 1940, nearly twenty thousand different occupations were listed by competent investigators.[7] A very significant example is the evolution of the social roles of scientists. No such roles existed in any society forty centuries ago; four centuries ago, hardly more than a dozen specialized roles could be distinguished, all of them in the domain of the natural sciences. But now there are about three hundred different varieties of specialists in natural and cultural sciences.

In investigating this evolution, we find, whenever evidence is available, that the development of every new variety of social role starts with the emergence of a *new personal function,* usually on the initiative of some individual innovator who gains a circle of followers or sponsors.

Another significant historical trend is the expansion of social circles. Judging from ethnological and archeological evidence, the great majority of social roles were originally performed within very limited social circles, including only those people with whom a particular person directly interacted; and many social roles are still so limited. But in the course of history the social circles of some persons who initiated certain functions expanded far beyond these limits and came to include thousands or even millions of people. This was possible whenever such a person gained assistants with auxiliary roles who acted as intermediaries between him and other people with whom he could not directly interact. These assistants compose what I call his "inner circle."

fluence of American education upon the later lives of educands. The main results of those studies and studies of education in other countries were summarized and published in Polish (*The Men of the Present and the Civilization of the Future,* 1934). My later studies of social roles remain unpublished except for *The Social Role of the Man of Knowledge,* Columbia University Press, 1940.

[6] Cf. Ralph Linton's two definitions in his book *The Study of Man,* 1936; some later modifications of his definitions; new changes introduced into the old definitions, based on class stratification; the definition of status as membership in organized groups; and the different conceptions used by investigators of social attitudes.

[7] H. Dewey Anderson and Percy H. Davidson, *Occupational Trends in the United States.*

Eventually, this inner circle can also expand and include assistants of assistants. I shall mention only briefly some instances of this expansion.

Historically the most important was the emergence of the roles of kings in Asia, Africa, and Europe, who with the aid of inner circles of military leaders, priests, and administrators assumed the function of integrating tribal communities into states. The emergence of the roles of dictators in recent times has been startling. Hitler, for instance, began with a circle of less than ten followers and became "der Führer" of millions of Germans within and outside of Germany. The communist dictators—Lenin, Stalin, and recently Mao—acquired very large social circles. Rather different has been the expansion of the social circles of the religious prophets, e.g., Mohammed and Luther. We are familiar with the expansion of the social circles of the famous men of letters, artists, and inventors. In the domain of economics, the vast expansion of the circles of employees and customers of men like John D. Rockefeller, Andrew Carnegie, and Henry Ford is instructive.

Since this expansion of social circles is still going on, a sociological theory of the modern world must include a comparative investigation of these dynamic processes in all kinds of social roles. While there are many thousands of individual biographies, no adequate general theory of such processes has yet been developed.

In my investigations of social groups during the last fifteen years, I omitted entirely "primary groups" in Cooley's sense, informal groups, and short-lasting groups, for studies of them contribute little, if anything, to the solution of the large-scale *new problems* which sociologists have to face. We concentrated on relatively large, organized social groups which last for a lengthy period of time.[8] A comparative study of the origin and development of many such groups indicates that nearly every one of them begins to emerge at the joint initiative of a few leaders for the common purpose of performing together a *collective function.* After the leaders have gained enough followers who are willing to cooperate in the performance of this collective function, the group becomes purposely organized, i.e., the cooperating individuals assume the roles of *members* whose personal functions are integrated. If the group is expected to be active for a long time, the social roles of at least some of its members become institutionalized.[9] This means that each of the specific personal functions which these members enact is considered so important for the lasting performance of the collective function of the group that there must always be somebody to enact it. When a particular individual for some reason ceases to do so, somebody takes it over—for example, the dean of a college; head of an administrative group, industrial manager, president or secretary of a scientific association. This does not imply, however, that he performs his role exactly as his predecessor did. Even in a relatively conservative group, individuals who enact institutional roles are allowed and even expected to do something new on behalf of the group, especially if the collective function of the group is being affected by external influences.

Many of the groups which we investigated did not start new functions; they merely reproduced the functions of other groups which already existed elsewhere—for example, public schools in the United States, local churches of an established religious denomination, and local sections of political parties. But new, functionally specialized varieties of social groups, just as new varieties of social roles, have been steadily evolving in the course of history and continue to evolve.

In preliterate societies, apart from clans, which are hardly specialized, relatively few organized groups with differentiated functions are found. I shall not try to enumerate the different varieties of social groups existing right now in American society. However, it is common knowledge that there

[8] See Florian Znaniecki "Social Groups in the Modern World" in *Freedom and Control in Modern Society* (D. Van Nostrand Company: New York, 1954, pp. 125–140), written in honor of Robert Morrison MacIver. There is considerable similarity between my theory of social groups and MacIver's theory of associations.

[9] See Florian Znaniecki "Social Organization and Institutions" in *Twentieth Century Sociology,* Gurvitch and Moore, eds., The Philosophic Library: New York, 1945.

are at least two hundred separate religious groups, each with some functionally specialized subgroups; about fifty ethnic varieties of immigrants and their descendants, with different cultures, each with a number of local and regional groups; many scores of clubs; a multiplicity of industrial and commercial groups; labor unions; a number of governmental groups; and numerous scientific, literary, and artistic associations.

In the course of this evolution, we see two distinct trends. First, the unification of a number of smaller groups, already existing or purposely formed, into larger groups under centralized control. This may be accompanied by functional specialization of certain subgroups within the larger group. A well-known example is the unification in modern times of numerous military groups, old and new, usually preceded or followed by some specialization of the military functions which the smaller groups perform. Another example is the unification of administrative or bureaucratic groups with increasingly specialized sections. Little specialization accompanied the unification of local Chambers of Commerce into a National Chamber. The development of political parties composed of regional and local groups did not lead to specialization by the latter.

The importance of this trend is obvious. The growing size of these groups results in their growing power. Compare the size and power of medieval military groups with such modern military groups as the German during the last two wars, or the American and the Russian right now. The German bureaucracy under the Nazi regime included four-and-half million persons. The federal bureaucracy of the United States rose in the last seventy years from a hundred thousand to about two million and a half. The connection between growth in size and growth in power has been studied.[10] So far as political parties are concerned, the most instructive example is the growing size and power of the Communist Party from the time Lenin and Trotzky returned to Russia up to this day, when the Communist Party within the Soviet Union exerts supreme control over the Communist Parties in most other countries.

A different historical trend is the gradual integration into complex societies of many *separate,* specialized groups, whose diverse original functions have been evolving independently. I have been trying to investigate the development of these societies from their early beginnings, believing that such an approach will eliminate the idea that a society has a static structure. I have already applied it to the evolution of modern national culture societies.[11] It can also be definitely applied to the evolution of political societies, ecclesiastical societies, and probably economic societies. And it should enable us to anticipate and plan the future formation of an integrated *world society* superimposed upon, but not substituted for, the many diverse contemporary societies.

I do not claim that I have solved any of the sociological problems mentioned here. But I insist that they must be solved, not only for the advancement of sociology as a science, but also to enable social leaders to collaborate in planning and promoting cultural creativeness and the social integration of humanity in order to prevent cultural destruction and social disintegration.

We know that it was the practical application of modern natural sciences, as they grew by new discoveries and developed new theories, which enabled technical leaders to plan and to achieve effective utilization of natural resources for human welfare and the protection of man against natural dangers. The task of sociology is much more difficult. Not only has its growth been slow and its influence upon social planning limited,[12] but the speed and range of new dynamic trends in the social domain have been increasing at a much faster rate than in the world of nature. If sociologists continue to go on as slowly as they do now, it will soon be too late to do any effective, large-scale social planning. Indeed, sociology itself may disappear completely, as it has behind the Iron Curtain.

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[10] See the *Annals of the American Academy of Political and Social Science,* Vol. 292, March, 1954, "Bureaucracy and Democratic Government," for several good studies of this problem.

[11] *Modern Nationalities,* University of Illinois Press, 1952.

[12] Cf. Florian Znaniecki "Sociological Ignorance in Social Planning," *Sociology and Social Research,* 30 (November–December, 1945), pp. 87–100.

THE FORMALIZATION OF SOCIOLOGY *

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THE term "formalization," as used in this paper, means the observance of a conventional set of rules in order to make explicit the logic of scientific proof. If a science is presented in this way, it is said to be "formalized"; otherwise it is "unformalized."

The advantage of formalization is that it makes it easier to judge the quality of the proofs offered. An incident from the life of Thomas Hobbes illustrates this. One day in a gentleman's library the philosopher noticed a copy of Euclid's *Elements* lying open at a certain theorem. Reading it, he exclaimed with an oath, "This is impossible!" but his curiosity led him to read the proof which referred him back to other propositions, and these, to others again. He continued until he saw clearly the whole logical ground for the proposition that had originally attracted his attention and he had to admit that the demonstration was sound.[1] The incident had a profound effect on the thought of Hobbes. For present purposes the point of the story is that formalized demonstrations can convince even a skeptical reader because their logical ground is displayed systematically and explicitly.

It would be unfair to contrast the formalized proofs of geometry with the unformalized proofs which sociologists usually offer for their assertions. Geometrical evidence is such by its very nature that it is easily formalized; it is vastly different from the evidence with which sociologists customarily deal. Nevertheless it is legitimate to ask whether some degree of formalization may not benefit sociology. In order to answer this question, it is necessary to consider, first, the logical ground for asserting sociological propositions and, second, certain systematic methods for making these logical grounds explicit, that is to say, methods for formalizing sociology. The remainder of this paper will be devoted to these two points.

GROUNDS FOR SOCIOLOGICAL PROPOSITIONS

The content of any science consists exclusively of propositions. Most frequently these propositions are expressed in ordinary prose, but they may be expressed equivalently in mathematical equations, tables, graphs, or otherwise. There must exist for each science criteria for deciding whether a given proposition is to be accepted into the science or rejected from it; if this were not the case, the content of the science would remain wholly indeterminate.

In any given science, two sorts of criteria are necessary for judging the acceptability of propositions, namely, criteria of relevance and criteria of scientific quality. Criteria of relevance are standards for deciding whether a given proposition falls within the scope of a particular science. Setting up criteria of relevance is therefore equivalent to defining the field covered by a science. None of us would hesitate to reject from sociology the following proposition: "The planes of cleavage in amphiboles intersect at angles of 56 degrees and 124 degrees." The proposition does not fall within the field of sociology as we conceive it. It is irrelevant to sociology, though relevant to mineralogy. The present paper will not further discuss criteria of relevance.

Criteria of scientific quality are standards for deciding whether a given proposition has the characteristics necessary for admission into a corpus of propositions which

* Paper read at the annual meeting of the American Sociological Society, September, 1954.

[1] This story is told by John Aubrey, a contemporary of Hobbes. See, *"Brief Lives," Chiefly of Contemporaries, Set Down by John Aubrey,* edited by Andrew Clark, Oxford: Clarendon Press, 1898, vol. 1, p. 332. The theorem that attracted Hobbes' attention was proposition 47 of book I, the famous Pythagorean theorem.

we call a "science." Although there is less than universal agreement among sociologists on the full list of criteria of scientific quality applicable to their science, most would probably agree, for example, that unproved guesses or unsupported theories lack scientific quality and hence do not qualify as propositions of sociology.[2] The present paper will attempt to avoid controversial issues and to discuss only those criteria of scientific quality that are fairly generally accepted.

Different criteria of scientific quality are applicable to different types of sociological propositions. It will be convenient here to distinguish three types, namely, postulates, empirical results, and theorems, and to discuss each of these types separately.

(1) A *postulate* of sociology is a proposition occurring within the science, but not proved within the science. Such a proposition may be proved within some other science or it may be acceptable without proof. The statement that the postulates of sociology are not proved within the science is equivalent to the statement that the sociologist does not apply criteria of scientific quality to his postulates. However, the sociologist may be responsible for showing that a certain postulate has been proved within some other science. Suppose, for example, that he introduces the proposition, "There is no known primitive society in which the family does not exist," as a postulate taken over from anthropology. In this case he does not prove the proposition by his own original work. He does not apply to it his own criteria of scientific quality. Nevertheless, he still has the responsibility for showing that the proposition actually is one belonging to the corpus of propositions which constitute the science of anthropology.

(2) An *empirical result* of sociology is a proposition proved within the science by the direct study of the real world of men and events. Some empirical results are simple protocol sentences of the type, "John Doe was at place p at time t." However, the more important empirical results are conclusions of field studies, inductive findings of more or less generality. The criteria used in judging the scientific quality of empirical results include standards for adequate sampling, standards for constructing reliable measuring instruments, standards for minimizing response error, and similar criteria discussed in books on sociological research.

(3) A *theorem* of sociology is a proposition deduced from postulates, empirical results, or from other theorems of sociology. For example, from the major premise that "elders are generally more cautious than the young," and the minor premise that there is an "increase in the percentage of old people," one may deduce the theorem that "the note of conservatism in American life" will be emphasized.[3] If a theorem is to be accepted as a proposition of sociology, it must be shown, first, that the premises are themselves accepted propositions of the science and, secondly, that the inference is formally valid. Criteria for judging this latter point are found in books on logic where the rules of deduction are given.

VARIOUS METHODS OF FORMALIZATION

It is clear from the definition of "formalization," given at the beginning of this paper, that the process involves two essential elements. The first essential element is that the logic of scientific proof must be made explicit; the second is that it must be made explicit in a regular, systematic way, by the observance of a set of conventional rules. The advantage of this second element in the process is easy to see. When a writer uses a set terminology, when he displays his proofs in a prescribed form and in a conventional order, then it is easy for the reader to see what criteria of scientific quality are applicable and how they should be applied. In short, it is easy for him to judge whether the writer is meeting the standards of good scientific work.

It remains now to consider various methods of formalization, that is, various sets of conventional rules by which formalization

[2] In the present writer's terminology, it is the function of a separate science called "metasociology" to set up criteria of relevance and criteria of scientific quality for sociology. See, Paul Hanly Furfey, *The Scope and Method of Sociology*, New York: Harper and Brothers, 1953.

[3] The example is taken from, W. F. Ogburn and M. F. Nimkoff, *Sociology*, 2nd edition, Boston: Houghton Mifflin, 1950, p. 353.

may be accomplished. Actually, there are an indefinite number of different possible ways of formalizing sociology. Three will be considered here as examples and they will be described very summarily because space is limited.

(1) One very elementary way of formalizing sociology is to display propositions and their proofs in ordinary, connected prose, but with careful adherence to the principle that every element in the logical structure of the science is to be clearly stated and identified as such. Such a presentation would demand, among other things, that every postulate be explicitly stated instead of being tacitly assumed, that every empirical result be accompanied by enough background material to permit a judgment of its scientific quality, and that the premises from which every theorem is deduced be set down as such. A presentation of this sort can rightly be called "formalized" because the writer does not follow his own whim, but submits himself to a definite set of principles in selecting and arranging his material, although the principles are somewhat flexible. The minimum condition for this sort of formalization is that the discerning reader must be able to identify unambiguously the grounds on which each statement is asserted. It is a type of presentation that is rather common in the exact sciences. It represents a good minimum standard for the sociologist.

(2) A second type of formalization uses special, conventional arrangements of material, emphasized possibly by typographical devices, such as the use of different sizes and styles of type, special systems of headings and subheadings, indention, the numbering of paragraphs, and the like. A good example of this type of formalized presentation in sociology is furnished by the third chapter of Robin M. Williams' book, *The Reduction of Intergroup Tensions.*[4] At one point in this chapter, Williams lists nine definitions and sets them apart by indention and smaller type. Then he lists certain of the "most general postulates in social science," using again the same typographical devices. Later he sets down 102 propositions in numbered paragraphs, most of which would be classified as "empirical results" in the language of the present paper, and supports them by quotations from the relevant literature, the quotations being distinguished typographically from the rest of the text. A presentation of this sort is possibly less easy to read than uninterrupted prose, but its logical structure is much easier to discern. Perhaps it would be better if we sociologists were less concerned with fine writing and more concerned with making the logic of our proofs unambiguously intelligible.

(3) A third, and extreme, type of formalization makes a liberal use of symbols, that is, conventional signs representing terms, relations, propositions, or sets of propositions. This procedure has two great advantages. First, a symbol, being freshly defined, is free of the troublesome chance associations which usually accompany a word of the ordinary language. Second, the use of symbols permits an extreme conciseness of statement and displays logical relationships with unusual clarity, as is the case, for example, when an equation is solved in the conventional symbols of mathematics. At present symbolization is little used by sociologists. Perhaps the most striking exception to this statement is the method used by S. C. Dodd in his *Dimensions of Society.*[5] Probably most sociologists feel that Dodd's experiment was not conspicuously successful, but it was pioneer work and it should be judged with that fact in mind. Possibly a less extreme use of symbolization might prove more rewarding.[6] At least the wider use of symbols as aids to a formalized presentation of sociology is a matter deserving serious consideration.

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[4] New York: Social Science Research Council, 1947.

[5] New York: Macmillan, 1942.

[6] G. A. Lundberg, in his *Foundations of Sociology,* New York: Macmillan, 1939, pp. 109–115, has collected a few examples of the use of symbolization by sociologists; most of them seem a bit trivial. For an extreme example of the use of symbolization in the presentation of an empirical science, see Joseph H. Woodger, *The Axiomatic Method in Biology,* Cambridge: University Press, 1937, in which the author attempts to restate part of the content of biology in the symbols of formal logic.

CONCLUSION

In order that a corpus of propositions—sociology, for example—may qualify as a science, each proposition should be judged by certain criteria of scientific quality. To make this process easy, it is advantageous to formalize the science, that is, to display explicitly the logic of its proofs in accordance with a set of conventional rules. The benefit of doing so is that valid proofs are more convincing, and invalid proofs are less deceptive, when their formal structure is unambiguously evident. Therefore formalization should prove a useful means for making sociology more genuinely scientific.

ROLE CONFLICTS OF MILITARY CHAPLAINS *

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IN a complex society many institutions are required to satisfy the manifold needs and wants of man. This means that any given individual will play many social roles in the course of his lifetime, and at any given time will be playing a number of roles simultaneously. Each person has a "hierarchy of role obligations," [1] the relative positions of which are determined by the strength of the claims made upon him by the various institutions which compete for his loyalty. Therefore, his roles may not necessarily be in harmony with each other, and in some instances, where two or more institutions demand the first loyalty of a person, they may be directly antithetical.

Assuming a "drive toward consistency of self" on the part of human actors in social situations, the person who finds himself playing two such divergent roles will seek some means of resolving the conflict between them. In the writer's estimation, the types of solutions available can be reduced to three: (1) abandonment of one of the conflicting roles, (2) rationalization, which may assume many different forms, or (3) compartmentalization of role behaviors.[2] If and when these techniques fail, the individual will invent new patterns of behavior which are usually thought of as neurotic, aberrant or psychotic, depending upon the severity of the conflict and the social definition of the given situation.

In the summer of 1952 a small-scale study was undertaken in the San Francisco Bay area in an attempt to determine the applicability of the theory in a concrete situation. A population was chosen in which the conflict in roles is extremely severe, since the ends specified by the two major institutions which define their social roles are in some respects mutually exclusive. These were military chaplains.[3] Chaplains not only share the dilemma of the Christian in war time; [4] they also function as officers

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* Revised version of paper read at the annual meeting of the Midwest Sociological Society, April, 1954. The material for this article has been taken from the writer's unpublished doctoral dissertation, "The Role of the Military Chaplain," University of California, 1953.

[1] The writer is indebted to Jackson Toby for this terminology. See his "Some Variables in Role Conflict Analysis," in *Social Forces*, 30 (March, 1952).

[2] The reader will note both similarities and differences between these concepts and those used by other investigators. See Jackson Toby, *op. cit.*, and J. W. Getzels and E. G. Guba, "Role, Role Conflict, and Effectiveness," in *American Sociological Review*, 19 (April, 1954), pp. 164–175.

[3] In an effort to compare the chaplains' concepts of their roles with the informal expectations of some of their "audiences," questionnaires were distributed to veterans and enlisted men. The data thus obtained will form the basis of a subsequent paper.

[4] Jewish chaplains share the same dilemma, at least to some extent, as is indicated by the statement of David de Sola Pool ("Religion's Answer to a Troubled World," in *The Military Chaplain*, official publication of the Military Chaplains Association of the United States, Inc., Summer, 1951): "We may with utter sincerity proclaim the purpose of our taking up arms as a righteous one, but the bitter unescapable fact remains that war as an instrument even of man's noblest purposes is and ever will be irreconcilable with religion."

in both ecclesiastical and military organizations. As officers, they are responsible, in part, for the achievement of the ends specified. It was assumed that chaplains, being highly educated and much concerned with consistency of behavior, would have been acutely aware of the conflicts between military and religious ideology, and would have sought some equitable solution.

METHODOLOGY

The sample was small, consisting of thirty-six chaplains and thirty-five ex-chaplains (who had seen service in World War II)—seventy-one in all. (The Chief of Navy Chaplains and the Chief of Army Chaplains were also interviewed, although they are not included in the sample.) In order to avoid bias in the selection of the sample, an effort was made to include every chaplain and ex-chaplain in the San Francisco Bay Area. Such a sampling method, while it successfully excludes the bias of the researcher, does not necessarily guarantee a truly representative sample of the population being studied. Headquarters of the Sixth Army and the Twelfth Naval District are located in San Francisco, which means that there is a heavy concentration of high ranking officers there. Consequently, the sample of chaplains is somewhat biased in favor of the higher ranks, with over half of them holding the rank of major or lieutenant commander or above. Rank is more equitably distributed in the sample of ex-chaplains, with three-fifths of them in the two lower ranks. Moreover, it was not actually possible to interview every chaplain and ex-chaplain in the area. Although only one person refused outright to be interviewed, many of them found reasons to postpone the interview until the time limit for the research had expired. It would be safe to estimate that not more than three-fourths to four-fifths of the chaplains and ex-chaplains in the area were actually interviewed.

In spite of the difficulties encountered, the coverage appears to be quite broad. All the gradations of rank from first lieutenant to brigadier general are covered; members of nineteen religious denominations, including thirteen Catholics, three Jews and fifty-five Protestants, were included in the sample (Table 1); and by coincidence the sample of chaplains includes twelve members each of the Army, the Air Force and the Navy. Nevertheless, because the sample was small and because the study was confined to a limited area, it must be regarded as exploratory in nature, and any conclusions must remain tentative until they are corroborated by further study.

TABLE 1. RELIGIOUS AFFILIATION OF CHAPLAINS AND EX-CHAPLAINS IN THE SAMPLE

Denomination	Chaplains	Ex-Chaplains
Catholic	7	6
Jewish	2	1
Protestant		
African Methodist Episcopal		1
Assembly of God	2	
Baptist (American)	4	1
Baptist (Southern)	4	
Christian Scientist	1	2
Congregational	2	1
Disciples of Christ (Christian)		1
Episcopalian	1	4
Latter Day Saints (Mormon)	2	1
Lutheran (Missouri Synod)	1	3
Lutheran (United)		2
Methodist	6	5
Mission Covenant	1	
Presbyterian *		3
Presbyterian (USA)	3	2
Reformed Church in America		1
Salvation Army		1
Total	36	35

* Not otherwise identified.

The schedule consisted of fifty-seven questions, twenty-nine of which were "open-end." The remainder called for categorical replies. Questions were designed to bring the respondent face to face with the proposition that the role of military officer conflicts with that of minister of the gospel, and to elicit his self-concept with reference to various situations which chaplains regularly face or might conceivably be called upon to face.

There are some five major items in Christian philosophy from which role conflict for the military clergyman derives. These are the doctrines of love, of universal brotherhood, of peace, and of non-resistance to evil, and the commandment, "You shall

not kill." These doctrines (or ideals, if the word doctrine seems too strong) are manifestly incompatible with the aims of a nation at war. Therefore it is impossible for the Christian in military service to put them into practice. Moreover, the age-old problem of the relationship between church and state is at issue here—whether the state or the church shall demand the first loyalty of its followers. With these items in mind, the respondent was asked to express his conception of his role in various situations.

Hypotheses to be tested included the following: (1) that the position of the chaplain does lead to a conflict in roles for the incumbent of that office; (2) that the chaplain seeks to reconcile this conflict either through rationalization or through compartmentalization of role behaviors; (3) that rationalization of conflict in roles tends to strengthen the chaplain's role of military officer at the expense of his role of minister of the gospel; (4) that the chaplain serves as interpreter of the values of the military organization, helps resolve value-dilemmas of individual service men, and helps promote smooth operation of the military organization.

ROLE CONCEPTS RELATING TO MILITARY DUTY

Chaplain's conceptions of self with relation to military duty. An attempt was made to discover the factors which motivated the individual respondent in joining the military service. Most of the replies (75 per cent) indicated that motives were mixed, but assuming that the first motive mentioned is the primary one, 25 per cent were influenced primarily by patriotic motives, while only 10 per cent were influenced primarily by religious motives. Another 25 per cent had not analyzed their motives beyond a "desire to be of service." The remainder gave replies that were in general non-committal. These replies indicate that, for those clergymen who join the military service, the claims of the state are likely to take precedence over the claims of religion at the outset.

Over 85 per cent of the respondents said that they had enjoyed military service. The aspect most frequently mentioned as being enjoyable was, for ex-chaplains, fellowship with men in the service, mentioned 17 times, and for chaplains, freedom, mentioned 9 times. "Freedom" included freedom from the cares and difficulties of a civilian parish, freedom from financial cares, and from the controls exercised by a congregation with its boards of deacons, Ladies' Aid groups, and the like, and freedom to act, preach and live as one chooses. It is no doubt significant that of the nine chaplains who mentioned freedom as the most enjoyable aspect of military life, only one stated that he did not intend to make a career of the chaplaincy.

This fact suggests an interesting possibility for research in personality. It raises the question whether or not the desire to escape from the relatively circumscribed area, dogmatically speaking, of the local congregation into the relatively free atmosphere, dogmatically speaking, of the armed forces plays a large part in the decision of a clergyman to make a career of military service. The foregoing may be stated in the form of a hypothesis, although the instrument used in this study was not designed to discover data bearing on such a hypothesis. Such data as are available do not, in fact, support the hypothesis. Exactly half of the chaplains said that they intended to remain permanently in the military service. None of them, however, gave freedom as a reason for making such a decision. Instead, the reasons mentioned were security, the opportunity to render a service, the challenge offered by the military situation, personal satisfaction, the existence of a need, and a missionary call. When ex-chaplains were asked to give their reasons for not remaining in the military service nearly 40 per cent of them expressed skepticism as to the necessity or propriety of a military chaplaincy in times of peace. In view of the difference in attitudes expressed by chaplains and ex-chaplains the foregoing hypothesis appears to be tenable in spite of the lack of evidence.

Chaplains' conceptions of self with reference to other officers. Over 90 per cent of the respondents reported that they had not felt out of place as military officers, and a like percentage believed they had been accepted as equals by their fellow officers. Only a few expressed reservations concerning their acceptance, although there appeared

to be a good deal of uncertainty on the part of chaplains as to what was expected of them by their fellow officers. For instance, some felt that having a few drinks "with the boys" helped to establish rapport; others felt that they would lose esteem if they indulged. The drinking party presents a good test of the loyalties of a clergyman belonging to an anti-liquor denomination. (Many denominations, including Catholics, Jews, Episcopalians and Lutherans do not forbid social drinking, even for the clergy.) Three chaplains who were members of anti-liquor denominations reported that they did drink at parties. All three, incidentally, avowed their intention to make a career of the chaplaincy.

Chaplains' conceptions of self with reference to military authorities. With respect to military authorities, personal and impersonal, 72 per cent of the respondents reported at the outset that they had the full cooperation of their commanding officers. A closer analysis of the replies reveals, however, that at least 50 per cent of the chaplains and 40 per cent of the ex-chaplains had had experience with a non-cooperative commanding officer. There was considerable hesitancy and some attempts at evasion occurred when questions were asked concerning relations with commanding officers. There is good reason for such caution. Chaplains have been historically regarded by commanding officers as appendages to the military organization, not as integral parts. During World War II and subsequently, however, they have been urging their claims to recognition with increasingly greater success. At the present time chaplains participate actively in recruit training programs and in the disposition of conscientious objectors. Requests for transfer or discharge for reasons of hardship are reviewed by a chaplain, and chaplains participate in courts-martial and other disciplinary procedures with varying frequency. All of these activities might conceivably be resented by commanding officers as infringements on their prerogatives.[5] Chaplains appear to be optimistic about such relations, and no chaplain interviewed reported that his present commanding officer was uncooperative.

The practice of "going over the head" of a commanding officer is reserved for extreme cases, although the possibility (and sometimes the threat) of doing so serves as an effective weapon in the hands of chaplains for securing cooperation. Nineteen per cent of the chaplains interviewed reported that they had actually gone over the head of a commanding officer at some time or other.

Reactions to regulations, channels, and red tape, were mixed. Eighty-six per cent of the respondents said that they had been successful in circumventing red tape on some occasions, and 72 per cent said that they regarded it as their duty to accomplish things in the most expedient manner, yet 31 per cent of the chaplains and 66 per cent of the ex-chaplains said that red tape had not interfered with their ministry. Over half of the respondents denied that any conflict exists between military regulations and religious ideology, and three of them denied that military regulations involve any moral values. The technique of compartmentalization is involved in these situations. It appears that the chaplain regards himself as operating in a moral context while he is conducting religious services or performing other tasks which are associated in his mind with the work of a clergyman, and which are not covered by military regulations, but not while dealing with commanding officers, fellow officers, channels of communication, and in other non-religious situations which are covered by the regulations. In religious situations he is guided by religious values; in other situations by other values.

There was evidence of ambivalence concerning the question of military discipline. Eighty-three per cent of the respondents said that at some time or other they had tried to intercede for the defendant in disciplinary cases. There were, however, a great many unsolicited comments concerning such activity, as though the respondents were trying to disabuse the interviewer of any erroneous ideas he might have concerning the functions of a chaplain as the champion of enlisted men opposed to officers. Fourteen respondents specifically stated that they had

[5] There is some evidence that such resentment does exist, but it is rarely expressed overtly. Instances of covert opposition, generally expressed in the planning of training exercises, do occasionally crop up.

interceded only rarely and in cases of extreme injustice, and the majority indicated that in general they identified themselves with the military hierarchy on the question of discipline. Only two respondents appeared to fit the popular stereotype of the chaplain as the champion of enlisted men. Chaplains in general do not regard it as their duty to mitigate the harshness of military justice. Most of them would prefer to do nothing until after sentence is passed, then attempt to rehabilitate the prisioner by persuading him that justice had been done, that it was for his own good, and that he should profit by the experience.

On the other hand, when the question of enforcing the regulation concerning saluting arose, most of the respondents sided with enlisted men. When asked what they would do if an enlisted man failed to show deference to their rank, only 10 per cent replied that they would remind him of his status. The explanation for this is not far to seek. The salute is regarded by chaplains as extraneous—an area of indifference—and the failure of an enlisted man to salute is not perceived as a threat to his status as an officer. The court-martial, on the other hand, has traditionally been a sort of battle ground of enlisted men versus officers. If the chaplain were to sympathize too openly with enlisted personnel on such occasions he might lose caste with his fellow officers.

Chaplains' conceptions of self with reference to enlisted personnel. Relations with enlisted men are of great concern to chaplains, since enlisted men form the largest single audience toward which chaplains direct their behavior. All chaplains are aware that the fact that they are officers poses a barrier to primary relations with enlisted men.[6] By and large, chaplains are probably more conscious of rank than any other group of officers in the armed forces, mainly because of their ambivalent attitude toward it. The desire to become an integral part of the military hierarchy, to become accepted as one of the "fellows," is very strong.[7] On the other hand, a priest, minister or pastor is one who is set apart from the group—a leader of the flock, not just one of them. But a good leader must be accessible; he must not be too distant from his flock; he must be on good personal relations with those he is leading. A military officer, however, must not be familiar with his men. His ability as a leader is presumed to depend, in part, on his ability to keep at a distance from his men. The chaplain, being both a military officer and a clergyman, must somehow come to grips with the problem of carrying on an effective religious ministry for enlisted personnel and at the same time of retaining his status as an officer.

In an effort to minimize the difference between chaplains and enlisted men, numerous devices have been officially adopted, chief of which are: (1) the practice of addressing all chaplains by the title "Chaplain" rather than by rank, thus de-emphasizing the military status and emphasizing the clerical status of the chaplain; (2) waiving the rule against fraternization between officers and enlisted personnel in the case of chaplains, thus de-emphasizing the social distance between them; and (3) specifically stating that chaplains do not have command of troops, thus making it possible to argue that the chaplain is not really an officer after all, although the command of troops is not the usual function of any staff officer. A rationalization frequently used by the respondents in this study was that they held their rank for purposes of pay only. It is not generally known, even by chaplains, that chaplains were granted equal rank with other officers some years before they were granted equal pay and that the struggle for rank centered on such questions as the wearing of the uniform or who should occupy which stateroom on a battle ship.[8]

Nearly all of the respondents felt that if

[6] The higher the rank the higher the barrier. More than one field grade chaplain, (major or lieutenant commander and above) reported that prior to their attainment of the higher rank they had made a practice of looking the other way when an enlisted man approached, in order to avoid noticing his failure to salute. At their present rank, however, it was not necessary to cultivate such a blind spot, for the salute was always forthcoming as a matter of course.

[7] Only one respondent, when asked the question, "Do you believe that your fellow officers accept you as one of them?" replied, "I am not 'one of them.' I am a priest and an extremist. Catholic personnel regard me as 'Father.'"

[8] Clifford M. Drury, *The History of the Chaplain Corps, U. S. Navy*, Vol. I (NavPers 15807), U. S. Government Printing Office, Chapters 10 and 11.

the individual chaplain were adequate he could overcome the barrier posed by rank. Only twenty-one per cent felt that they could have done an effective job as chaplains if they had not had military rank, and no respondent believed that an enlisted chaplain could perform effectively. Three major reasons were advanced for such skepticism: (1) that without military rank it would be impossible to demand concessions for enlisted men from the military hierarchy, (2) that it would be impossible to minister to officers unless the chaplain were one of them, and (3) that if the chaplain were not an officer he could not command the respect of either enlisted men or officers. In spite of this, the usual response to questions suggesting a conflict between the demands of the chaplain's military office and those of his ecclesiastical office was to deny the existence of conflict by disclaiming the reality of his status as an officer.

ROLE CONFLICTS RELATING TO RELIGIOUS BELIEFS AND PRACTICES

Chaplains' conceptions of self with reference to religious life in the armed forces. Most chaplains curtail certain of their religious activities when they enter the military service. The prescribed duties of a chaplain do not include evangelism, and by a sort of gentlemen's agreement he is expected not to proselytize. Since the efficiency of a chaplain is not measured by the addition of members to his congregation, and since the line between evangelism and proselytism is ill-defined, many chaplains prefer not to emphasize this phase of religious activity. Conversions do occur, however. Only 10 per cent reported that they had never baptized military personnel.

Seventy-nine per cent of the respondents believed that a man with good religious training would make a better soldier than one who lacked such training, although only 30 per cent thought that an appeal to the religious motives of men was more effective in securing military efficiency than an appeal to patriotic motives. There appears to be some evidence of ambivalence here—on the one hand there is a tendency to urge the advantages of religious training (which, incidentally, reinforces the chaplains' claim to a permanent position in the military hierarchy), and on the other hand a reluctance to admit that religion can be so readily used in the pursuit of non-religious ends. All recognize the function of religion in the maintenance of morale, but they prefer to regard morale as a by-product of their spiritual ministrations rather than its major object.

Chaplains' conceptions of self with reference to the relations between church and state. In spite of the traditional American ideology of the strict separation of church and state, military chaplaincy is a state supported religious institution—not actually an official religion, but a form of state established church. It represents an odd situation, since the government specifically disclaims any interest in controlling religious beliefs, and permits individual chaplains or denominations to prescribe rituals, dogmas, and other religious matters. The only religious qualification demanded of an applicant for the chaplaincy is the recommendation of his denomination (or of an officially recognized ecclesiastical agency which reviews applications for the chaplaincy). There are, however, some rather strict educational requirements which the military services impose for reasons of their own. These requirements have the effect of defining orthodoxy, for they exclude from participation a great number of clergymen who are considered by their denominations sufficiently qualified to carry on the ministry of the church. When asked for their reactions to the statement that the chaplaincy is a form of state established church, 68 per cent of the respondents disagreed, more or less violently. A greater proportion of chaplains than of ex-chaplains disagreed (81 per cent as opposed to 54 per cent), and with greater vehemence, arguing that the presence of many denominations and the failure of the state to recognize any denomination as official preclude a state establishment of religion.

Chaplains' conceptions of self with reference to the question of religion and war. The question of religion and war offers to chaplains even greater difficulty than the question of rank. Various passages in the Bible appear to forbid the use of violence in the pursuit of ends. However, 45 per cent of the respondents believed that the killing of an

enemy soldier was a righteous act and the remainder called it a justifiable act. None felt that the individual soldier had any moral responsibility in the matter except to serve his country—a duty which in time of war takes precedence over all others. Only 7 per cent ascribed any moral content to the act of killing in war time, and these would distribute the guilt over the entire nation. The assumption of a "just" war (or even of a "holy" war) was the argument most frequently used in defense of war-time killing, followed closely by the plea of self-defense. It was argued further that the commandment "You shall not kill" has been corrupted in translation (a matter concerning which Bible translators appear to disagree) and that when it is read according to the ancient Hebrew tribalistic interpretation, "You shall not murder," no moral wrong is involved in war-time killing.

When asked to reconcile the "turn the other cheek" philisophy with war, one-fifth of the respondents felt that the conflict was so great that it could not be reconciled; with a few exceptions the remainder felt that there was no conflict since: (1) the individual and nation are different, (2) the necessity for self-defense obviates any anti-violence teachings of Jesus, (3) "turn the other cheek" needs interpretation, and (4) this philosophy does not apply in this day and age. There were three respondents who said that they had never faced the question and who refused to face it during the interview.

The practice of giving theoretical adherence to a doctrine while violating it in practice is as old as religion itself, and it poses a major problem for moralists in every age. It accounts, in part, for the Christian's continuing sense of guilt or unworthiness. For the Christian to deny the relevance of the admonition to turn the other cheek is to question the value of the ethical teachings of Jesus; to admit it makes the Christian in military service a violator of his own moral precepts. The position actually taken on this question by most of the respondents was that of moral relativism.

None of the respondents would of his own volition raise questions concerning the morality of killing, or of war in general, or of turning the other cheek, or any question concerning the relationship between religion and war. If such questions came up in the course of a discussion, the majority would attempt to reason them out on a common-sense basis rather than on a religious basis. Many respondents had not attempted to answer such questions even in their own minds prior to the interview—a fact which violated one of the assumptions made at the outset of the study, namely, that chaplains, being educated men and philosophers who are concerned with the consistency of self, would have sought solutions to such a dilemma. Instead, it appears that the greater the dilemma, the greater the tendency to withdraw from it—to compartmentalize role behaviors and to refuse to recognize conflicting elements.

CONCLUSIONS

The foregoing report gives only a bare outline of a portion of the data actually collected. These data tend to support the hypotheses advanced, in some respects strongly, in other respects less strongly. The first hypothesis, that the position of the chaplain does lead to a conflict in roles for the incumbent of that position is supported in two ways: (1) by a philosophical analysis of the two social roles of clergyman and military officer and (2) by the responses to questions in the interview schedule, particularly those questions dealing with rank and those dealing with the relationship between religion and war.

The second hypothesis, that the chaplain seeks to reconcile his role conflict either through rationalization or through compartmentalization of role behaviors, appears to be well substantiated. The sample chosen did not include any member who had sought to escape the conflict by abandoning one of the roles (although one ex-chaplain had abandoned the ministry after he had returned to civilian life). Therefore, only two escapes were open. Compartmentalization appeared to be the more successful of the two techniques, and more frequently used. Rationalization requires facing the dilemma and arguing away the conflicting elements. Compartmentalization involves refusing to recognize the conflict. Perhaps one respondent was speaking for more than himself when he said that, although he had been a strong

pacifist prior to World War II, when he saw the great need of men in the military service, he forgot about philosophy and resolved to help as best he could in an unfortunate situation.

Concerning the third hypothesis, that rationalization of conflict in roles tends to strengthen the chaplain's role of military officer at the expense of his role of minister of the gospel, it needs only to be pointed out that every argument cited tends to assert the military claim and de-emphasize the religious claim.

The fourth hypothesis, that the chaplain serves as interpreter of the values of the military organization, helps resolve value-dilemmas of individual service men, and helps promote smooth operation of the military organization, is less strongly supported than the others, partly, perhaps, because of a lack of data bearing on the subject. However, such data as are available indicate that the hyphothesis is tenable. If a service man were in doubt about the morality of military activity, a majority of the respondents would endeavor to assure him that his relationship with God would not suffer thereby. The recently instituted "Character Guidance Program" [9] is a direct attempt on the part of chaplains to "sell" military life to service men. Moreover, all the chaplains' welfare activities are designed to help the service man adjust more smoothly to the military situation.

It would be over-hasty to generalize on the basis of the data in this study. However, two hypotheses may be suggested: (1) that the role which provides for the individual his primary identification takes first place in his hierarchy of role obligations and (2) that for the chaplain the role of military officer provides his primary identification. Further study along this line would no doubt reveal much concerning techniques of resolving role conflicts.

The role of the military chaplain offers an interesting subject for the student of role conflict. The chaplaincy is still in its formative stage, and we are witnessing here the emergence of a new social role in which the ideological clash between church and state is assuming what is, for the United States, a new form. The role of the military chaplain also offers an interesting subject for the student of personality. The instrument used in this study was not designed to reveal the individual personality characteristics of the subjects. Nevertheless, the replies to some of the items in the interview schedule (for instance, the question concerning the enjoyable aspects of military service, discussed above) suggest the existence of basic personality differences between those clergymen who choose the military chaplaincy as a career and those who do not. The hyphothesis that a career in the military chaplaincy appeals more strongly to those possessing the characteristics of the "authoritarian" personality than to other types would be in order here. The student of institutional history and of the processes of social change should also find much of value in the study of the role of the military chaplain. The history of military chaplaincy, as an institution, covers several centuries, but its origin can be dated, and the various steps in its development can be located in time and space.

[9] The Character Guidance Program is a program of instruction in "godly patriotism," which attempts to equate the values of patriotism and religion by giving patriotism a religious basis. Recruits are required to attend a certain number of Character Guidance lectures during their basic training. All three services have adopted the program, and the ultimate aim is to require every enlisted person in the armed services to attend one Character Guidance lecture per month throughout his (or her) tour of duty. The lectures are "canned." They are prepared by a special Board of chaplains, and prior to being distributed to chaplains in the field, they are approved and pronounced official state policy by the Secretary of Defense.

REPEATED MIGRATION AS A FACTOR IN HIGH MOBILITY RATES

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THE United States has been characterized as a land of migrants. The 1950 census found 5.6 per cent of the population living in 1950 in a county different from that in which they were living in the previous year.[1] In a sample survey which the Bureau of the Census made in 1947, it was found that 21 per cent of the population was living in a county different from that in which they had been living in 1940.[2] While these data lend statistical support to the characterization of the American population as being highly mobile, they do not permit the determination of the extent to which the high mobility rates suggested by these and other surveys are the product of the repeated movements of the same persons from place to place, or the single moves of a larger number of persons from just one place to another where they become continuous residents. On the local community level this problem raises the question of whether high rates of in- and out-migration indicate a high degree of population instability, or if instead they represent the movement into and out of the community of the same migrant elements in the population, indicating thereby that there remains a large segment of the population which by its continuous residence in the community provides continuity and stability to the basic population and to the social organization.

As part of a larger population study of Norristown, Pennsylvania, a medium-sized community twenty miles northwest of Philadelphia, an analysis was made of the patterns of in- and out-migration in each decade from 1910 to 1950. This analysis indicated that in each of these decades, a minimum of one-third of the adult male population resident in Norristown at the beginning of a decade had moved out by the end of that decade; and that at least one-third of the adult male population resident in Norristown at the end of each decade had moved into Norristown during the course of the previous ten years.[3]

Such a high rate of mobility could easily be interpreted as indicative of a high degree of population instability in Norristown. In an attempt to test the validity of such an interpretation, the Norristown data upon which the migration analyses were based were subjected to further analyses to determine the extent to which the out-migrants of one decade were the in-migrants of the previous decade. This report on the methodology and the results of the investigation will show (1) how in the absence of a continuous population register such as exists in Sweden, other local sources of data can be used to yield demographic data which are otherwise unobtainable in the United States, and (2) how, through the use of these data, it becomes possible to measure the extent to which the volume of migration is a product of the repeated movements of selected persons rather than the single moves of a larger number of persons in the total population group.

METHOD

The analysis of migration in Norristown from 1910 to 1950 was based on the coordinated use of city directory data, vital statistics information, and school records. The city directories, which have been published biennially for Norristown since 1860, contain an alphabetical listing of all the adult

[1] Bureau of the Census, "Mobility of the Population—State of Residence in 1949 and 1950," *1950 United States Census of Population: Advance Reports*, Series PC-14, No. 17, July, 1953, p. 5.

[2] Bureau of the Census, "Internal Migration in the United States: April 1940 to April 1947," *Current Population Reports*, Series P-20, No. 14, April, 1948, p. 1.

[3] Sidney Goldstein, *Patterns of Internal Migration, Morristown, Pennsylvania, 1910–1950*, unpublished doctoral dissertation in sociology, University of Pennsylvania, 1953, pp. 202–211.

males[4] in Norristown and information on their occupations. Tests made of these directories by comparing their enumerations with comparable United States Census data indicated that they afford the demographer an accurate source of data on both the total size of the adult male population and its occupational composition.[5] The method by which these directories were utilized in conjunction with vital statistics and school records to obtain data on the patterns of migration was a comparatively simple one and was a necessary preliminary to the utilization of these same data for the particular investigation being reported on here. For purposes of summarizing the procedure, the 1940–1950 decade will be used as an example. The same procedure was followed for each of the three other decades, 1910–1920, 1920–1930, and 1930–1940.

Two samples were drawn from the directories of that decade, one from the 1940 directory and the other from the 1950 directory. Each sample consisted of a ten per cent random selection of all the Norristown male residents listed in the directories and therefore represented the male population of Norristown eighteen years of age and over in 1940 and 1950, respectively.

The first of these samples, that drawn from the 1940 directory, served as the basis for analyzing migration out of the community during the ensuing decade. All of these people were living in Norristown in 1940. Their names were traced through all the succeeding directories of that decade up to and including that of 1950. A number of them were found in all the directiories, but others disappeared in the course of being traced. Not all of these disappearances were attributed to migration, inasmuch as some of them had died since the last directory census. Therefore all the disappearances were cross checked in the Norristown death records, and only after those who had disappeared due to death had been identified was the remaining loss then attributed to out-migration.

The second sample, that drawn from the 1950 directory, served as the basis for determining migration into the community during the previous decade. All of these people were living in Norristown in 1950. Their names were traced back through all the preceeding directiories of the decade up to and including that of 1940. As in the case of the 1940 sample, some of these persons were listed in all the directories. Others did not appear as they were traced back in time. These were the potential in-migrants—potential because it was also possible that some were not found in the earlier directories because they were under the eighteen year minimum age required for inclusion in the directory listings. After these eighteen year olds had been identified through the use of school records and birth cirtificates, the remaining group of persons who did not appear in the earlier directories were classified as in-migrants to Norristown.[6]

Each of the samples chosen from the 1920, 1930, and 1940 directories was used as the basis for identifying both the in-migrants of the previous decade and the out-migrants of the next decade. This procedure therefore provided information covering each person in each of the three samples for a twenty-year period, since the persons from the 1920 directory sample had been traced back to 1910 and forward to 1930, those in the 1930

[4] Until 1930 the directories classified persons 21 years of age and over as adults. After 1930, persons 18 years of age and over were so classified.

[5] Goldstein, *op. cit.*, pp. 148–202.

[6] The results yielded by this method of identifying the in- and out-migrants in the Norristown population were subjected to a variety of tests to measure their accuracy. These tests showed (1) that directories provided a complete enumeration of the adult male populations; (2) that death records could be used with great success to identify those persons who had disappeared from the city directories due to death rather than to out-migration; and (3) that school records and birth certificates could be successfully employed to identify those who first appeared in directories upon becoming eighteen. The accuracy of these sources of data gave assurance that those disappearances from the directories that could not be accounted for by death could correctly be attributed to out-migration; and similarly, that those new appearances that could not be attributed to the eighteen-year-olds could accurately be ascribed to in-migration. The use of the method of residues to identify in- and out-migrants was justified, therefore, in that the number of errors included in the migrant category were minimal. Consequently the rates of in- and out-migration accurately represented the migration experience of the Norristown population. For a detailed discussion of the results of these tests of accuracy see, Sidney Goldstein, "City Directories as Sources of Migration Data," *American Journal of Sociology*, (September, 1954).

sample back to 1920 and forward to 1940, and those in the 1940 sample back to 1930 and forward to 1950.

In the forward tracing process each person was ultimately identified as either (1) a continuous resident of Norristown, (2) a person who had died, or (3) an out-migrant. In the backward tracing process each person was ultimately identified as either (1) a continuous resident of Norristown throughout the previous decade, (2) a person who first appeared in the directory because he reached the minimum age, or (3) an in-migrant. By matching the forward and backward tracing record of each person in the sample, it thus became possible to assign each person in each of the three samples to one of the following nine categories.

1930–1940 *Backward Tracing*	1940–1950 *Forward Tracing*
1. Resident	Resident
2. Resident	Died
3. Resident	Out-migrant
4. 18 Year Old	Resident
5. 18 Year Old	Died
6. 18 Year Old	Out-migrant
7. In-migrant	Resident
8. In-migrant	Died
9. In-migrant	Out-migrant

Thus the basic data were available for determining the previous residential status of out-migrants and for testing the hypothesis that out-migrants tended in large measure to be the same persons who were previously in-migrants. This could be demonstrated by showing that the out-migration rate for the in-migrants was significantly greater than the out-migration rate for persons who had been continuous residents of Norristown in the previous decade, and conversely that the in-migration rate of the out-migrants was significantly greater than the in-migration rate for persons who were continuous residents of Norristown in the following decade.

RESULTS

The 1940 sample of the adult male population of Norristown numbered 1,153 persons. Of this total, 47.4 per cent had been resident in Norristown throughout the 1930–1940 decade, 37.2 per cent had moved into Norristown between 1930 and 1940, and 15.4 per cent had entered the adult male population group by attaining their eighteenth birthday during the previous decade. Of this same total of 1,153 persons, 52.8 per cent continued to be residents of Norristown throughout the entire succeeding decade, 1940–1950, 37.7 per cent moved out of Norristown during that ten year interval, and 9.5 per cent died. The data for both these decades are characteristic of the large amount of population movement into and out of Norristown throughout the last forty years.

The data in Table 1a indicate the experience of the migrants and residents over the twenty year period of time represented by the 1930–1940 and 1940–1950 decades. Among the 546 persons who were resident in Norristown throughout the 1930–1940 decade, 61.5 per cent continued to be resident in Norristown during the following decade 1940–1950. Of the remainder, 15.8 per cent died and 22.7 per cent migrated out of Norristown. In contrast, of those persons who were migrants to Norristown during the 1930–1940 decade, only 44.1 per cent continued as residents of Norristown throughout the ensuing decade. A small number, 4.4 per cent, died. The largest group, 51.5 per cent, had moved out of Norristown by the end of the decade following that in which they had moved into town.

These data confirm the hypothesis that the out-movement from Norristown consisted in large measure of persons who had moved into Norristown in the earlier decade. Of every 100 persons who had entered

TABLE 1a. PERCENTAGE DISTRIBUTIONS BY MIGRATION STATUS 1940 TO 1950 OF MALES WHO WERE CONTINUOUS RESIDENTS OF NORRISTOWN 1930 TO 1940 COMPARED WITH THOSE WHO IN-MIGRATED

	Residents 1940–1950	Deaths 1940–1950	Out-Migrants 1940–1950	Total Per Cent	Total Number
Residents 1930–1940	61.5	15.8	22.7	100.0	546
In-Migrants 1930–1940	44.1	4.4	51.5	100.0	429

Norristown between 1930 and 1940, 52 had left between 1940 and 1950. On the other hand, of every 100 persons who had been resident in Norristown throughout the 1930–1940 decade, only 23 had left between 1940 and 1950. Thus the out-migration rate of the in-migrants was over twice that of the "continuous residents" of Norristown.

This same point can be shown by examining these same data from a different perspective, as is shown in Table 1b. Of all the persons who out-migrated from Norristown in the decade 1940–1950 only 28.6 per cent had been residents of Norristown throughout the previous decade, 20.5 per cent had turned 18 in that earlier period, and 50.9 per cent had been in-migrants to Norristown. Contrasted to this, of those who were resident throughout the 1940–1950 decade, 55.2 per cent had also been resident throughout the previous decade, 13.8 per cent had turned eighteen, and 31.0 per cent had been in-migrants. Once again the figures indicate the higher degree of stability among the persons classified as "residents" in either of the two decades than among those classified as migrants.

Table 1b. Percentage Distributions By Migration Status 1930 to 1940 of Males Who Were Continuous Residents of Norristown 1940 to 1950 Compared With Those Who Out-Migrated

	Residents 1940–1950	Out-Migrants 1940–1950
Residents 1930–1940	55.2	28.6
In-Migrants 1930–1940	31.0	50.9
18 year olds 1930–1940	13.8	20.5
Total per cent	100.0	100.0
Total number	609	434

The data on the migration "histories" of the resident and migrant groups living in Norristown in the two earlier twenty year periods, 1920–1940 and 1910–1930, are presented in Tables 2 and 3. Since the patterns of stability and continuous movement found to characterize the residents and the migrants of the 1930–1950 time span were also characteristic of the population of these two earlier periods, no separate discussion of them will be undertaken here. They serve to reinforce the earlier finding that there is a significantly higher rate of out-migration among in-migrants than among those classified as "continuous residents," and show in addition that this has been true of Norristown throughout the forty year period under investigation.

Table 2a. Percentage Distribution by Migration Status 1920 to 1930 of Males Who Were Continuous Residents of Norristown 1930 to 1940 Compared with Those Who Out-Migrated

	Residents 1930–1940	Out-Migrants 1930–1940
Residents 1920–1930	48.1	27.1
In-Migrants 1920–1930	40.5	60.2
18 year olds 1920–1930	11.4	12.7
Total per cent	100.0	100.0
Total number	553	425

The importance of these findings for Norristown and for other migration studies lies in the fact that they have demonstrated that high rates of in- and out-migration do not necessarily mean a correspondingly high degree of population change or population instability. If, as the data indicate,

Table 2b. Percentage Distributions by Migration Status 1930 to 1940 of Males Who Were Continuous Residents of Norristown 1920 to 1930 Compared with Those Who In-Migrated

	Residents 1930–1940	Deaths 1930–1940	Out-Migrants 1930–1940	Total Per Cent	Total Number
Residents 1920–1930	57.2	18.1	24.7	100.0	465
In-Migrants 1920–1930	43.6	6.6	49.8	100.0	514

a high proportion of the out-migrants were former in-migrants, then there was no general turnover in the total population of Norristown but only in a certain segment of that population—the migrant group. While migrants came and went, a large segment of the population continued to remain in the community throughout the entire twenty years and thereby provided a high degree of stability to the population of the community.

TABLE 3a. PERCENTAGE DISTRIBUTIONS BY MIGRATION STATUS 1910 TO 1920 OF MALES WHO WERE CONTINUOUS RESIDENTS OF NORRISTOWN 1920 TO 1930 COMPARED WITH THOSE WHO OUT-MIGRATED

	Residents 1920–1930	Out-Migrants 1920–1930
Residents 1910–1920	58.8	33.4
In-Migrants 1910–1920	35.8	56.1
18 year olds 1910–1920	5.4	10.5
Total per cent	100.0	100.0
Total number	422	362

This finding has significance to the demographer as well as to the student of social change and social disorganization. It suggests that the large volume of movement which has been shown to characterize the American population may be attributable in large measure to the repeated movements of a small number of persons rather than to the single moves of a larger proportion of the population. It has been suggested, in fact, that from the point of view of the significance of migration to cultural and social change, it might be wise to distinguish in our population between the settled residents and the nomads; those who tend to remain stationary in one locality and those who tend to make repeated moves from one place of residence to another.[7] The fact that the out-migrants from a community tend to be in large measure the in-migrants of an earlier year suggest that despite a high migration rate, there is available in the continuous residents a core population group which gives stability to what otherwise might be a highly unstable social organization and which insures a continuity of the system of social values existing in the local community. Since the continuous in- and out-movement is composed largely of the same persons, the great majority of migrants must at any given point in time be somewhat marginal persons in the community with little interest in or time available for integration into its core social organization. The fact that only a minority of the in-migrants of one decade remain on as continuous residents throughout the next decade means, too, that the absorption of these persons of different origins and different values into the core population of the community may not have as disorganizing an effect as would be the case if the proportion of migrants who remained in the community were larger. At the same time, however, the very fact that these migrants are not integrated into the community and that the social values held by these persons may be very different from those of the resident population provides the basis for cultural conflict between the two groups.

Lack of integration into the core population of the community does not, however, mean that the contact of the nomads with the settled residents has no effect on their own

[7] Analysis of the occupational composition of the nomad segments of the Norristown population indicated that these persons came from both the white collar and the blue collar laboring groups and not just from a few selected occupational categories.

TABLE 3b. PERCENTAGE DISTRIBUTIONS BY MIGRATION STATUS 1920 TO 1930 OF MALES WHO WERE CONTINUOUS RESIDENTS OF NORRISTOWN 1910 TO 1920 COMPARED WITH THOSE WHO IN-MIGRATED

	Residents 1920–1930	Deaths 1920–1930	Out-Migrants 1920–1930	Total Per Cent	Total Number
Residents 1910–1920	56.9	15.3	27.8	100.0	436
In-Migrants 1910–1920	39.4	7.6	53.0	100.0	383

beliefs, attitudes, and activities or on the beliefs, attitudes, and activities of the resident population. In the process of social interaction arising out of living and working in each other's midsts, each group will no doubt modify the other in varying degrees, depending in large measure on the length of time over which the two groups are in contact. To the extent that such modification takes place, these migrant persons in the population serve an additional important function as agents of cultural change. Because they experience repeated changes of residence, they come in contact with persons of different beliefs and attitudes. Being bearers of culture themselves and having their own attitudes and beliefs modified by their varied contacts suggests the hypothesis that the migrants play an important role as culture diffusers, for in the process of moving from one place to another they carry with them not only their own way of life, but in varying degrees and in modified forms the way of life of those persons with whom they had come in contact in all their earlier movements. Thus, in turn, every time they come into contact with new groups in new places they probably have some impact on the way of life of these resident populations.

These data suggest that the old concept of a melting pot is still useful in viewing the American cultural scene. In the past, this term was applied to the process of acculturation resulting from the mixture of the foreign-born and the native-born. The present analysis suggests, however, that such a conception of the melting pot may have obscured another important process of acculturation which has characterized the United States throughout its history—that resulting from the continuous movement of the nomads in our population who, by carrying with them some of the culture traits of the diverse places where they lived, served to diffuse these traits and thereby to contribute to the creation of a more homogeneous American culture.

TRENDS IN OCCUPATIONAL ORIGINS OF BUSINESS LEADERS *

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IN the recent literature on social mobility, the hypothesis that American business leaders are showing increasing social inheritance appears firmly established. Several major studies have now been devoted to the mobility rates and inheritance patterns of this occupational group.[1] Virtually all these studies, when regarded as tests of the hypothesis, have resulted in apparent confirmation. The results have ranged from the very specific, such as Taussig and Joslyn's ". . . prominent business men are coming increasingly from families in which the father was a business man,"[2] to the highly general, such as Rogoff's, ". . . the processes by which men . . . were selected for occupations were more closely related to social origins in 1940 than in 1910."[3]

In view of this apparent convergence of findings, it may seem needless to inquire further into business leader origins at the

* The writer wishes to acknowledge his indebtedness to the Development Fund of Ohio State University for assistance which made possible the field work for this study.

[1] Pitirim Sorokin, *Social Mobility*, New York: Harper and Bros., 1927; F. W. Taussig and C. S. Joslyn, *American Business Leaders*, New York: Macmillan Co., 1932; Percy E. Davidson and H. Dewey Anderson, *Occupational Mobility in an American Community*, Stanford: Stanford University Press, 1937; Robert and Helen Lynd, *Middletown in Transition*, New York: Harcourt, Brace and Co., 1937; C. Wright Mills, "The Middle Classes in Middle-Sized Cities," *American Sociological Review*, 11 (April, 1945), pp. 242–249; W. Lloyd Warner and associates, *The Social System of the Modern Factory*, New Haven: Yale University Press, 1947; Natalie Rogoff, *Recent Trends in Occupational Mobility*, Glencoe: Free Press, 1953.

[2] Taussig and Joslyn, *op. cit.*, p. 234.

[3] Rogoff, *op. cit.*, p. 106.

present time. Yet there are substantial reasons, apart from the standing need for replication, for new studies in this area. Much of the pertinent research is now old, and new insights and techniques have become available. There have been major shifts in the power structure of this society since the first of these studies, and the altered status of business leaders may be reflected in changing mobility rates. Finally, there is growing controversy over conclusions regarding the rigidity of class lines in America, which in effect opens to fresh scrutiny all generalizations in the field of social mobility.[4]

This report deals with a narrow segment of business leader mobility, involving selected general correlates of leader occupational origins. Three problems are considered: (1) Are there significant regional differences in occupational origins of business leaders? (2) Is there a discernible secular trend in occupational origins of business leaders? (3) Is there a determinate relationship between the variables of business size and occupational origin?

The choice of these particular independent variables is in part a function of their prominence and in part a matter of convenience. Neither time nor the character of the present data permit dealing with such factors as motivation or personal capacity, or with social-structural variables such as organizational maturity or business type. It is hoped that comprehensive studies dealing with a wider range of factors will be possible at an early date.

PROCEDURES

Data on business leaders were obtained by a combination of mailed questionnaires and follow-up interviews with selected non-respondents. The questionnaires were mailed to 510 "business owners or executives employing 100 or more persons" in two geographical regions—Worcester and Springfield, Massachusetts, and Columbus and Toledo, Ohio. These cities had been selected for representativeness of the occupational profile of gainfully employed urban males in their respective states. Names of prospective respondents were drawn from directories of business heads supplied by chambers of commerce in the sample cities. Selection of the heads of larger businesses was simplified by the fact that listings were in terms of number of employees.

The questionnaire covered the basic social and economic histories of both the respondents and their fathers. A code number on each questionnaire permitted identification of respondents, thus making possible later interviews with particular individuals in each of the sample cities. Interviews with non-respondents were planned both as a check on bias in the mail returns and as a means of increasing the number of returns.

Approximately thirty-five per cent of the sample list returned completed questionnaires. This percentage was approached closely in each of the four cities. Upon cessation of the mail returns, a one-third random sample of non-respondents was interviewed in the two Northeast cities. Original plans had called for follow-up interviews in both the Northeast and Midwest, but time was not available for the latter. A total of 220 usable returns was obtained: 102 in the Midwest, and 116 in the Northeast. Of the latter, 42 returns were obtained by interviews, which were divided equally between the two cities in that region.

The central concept in this study —"occupational origins"—was defined as "the principal occupation of the respondent's father." The principal occupation was ascertained by direct inquiry in the questionnaire and interview, and verified by several related questions. Following some open-end items on the father's principal occupation, for example, the respondent was asked to locate the occupation in a checklist consisting of a number of graded series of occupations. Further questions dealt with number of persons employed or supervised by the father, and with secondary occupations followed.

To facilitate statistical operations, the principal occupation of the respondent's father was scored on the North-Hatt scale

[4] See, for example, Gideon Sjoberg, "Are Social Classes in America Becoming More Rigid?" *American Sociological Review*, 16 (December, 1951), pp. 775–783; also, Gilbert Burck and Sandford Parker, "The Changing American Market," *Fortune*, 48 (August, 1953), pp. 98 ff.

of occupational status.[5] Occupational origin was thus transformed into a numerical value on a scale which ranges from 33 points for "shoe shiner" to 96 points for "Supreme Court justice." The North-Hatt scale was chosen in preference to the Edwards or the Bureau of the Census occupational "scales" for a number of reasons. Being an interval or quasi-interval scale rather than a nominal or partially ordered scale, it permits greater range and flexibility in analysis..[6] Beyond this, it eliminates the problem caused by temporal or regional shifts in the occupational profile, since it consists of abstract ratings, not occupational categories. One logical objection to the use of the North-Hatt scale, the possibility that it does not correctly represent statuses of a generation ago, was adjudged to be unfounded. Rank order correlations between identical occupations in the Counts[7] and North-Hatt scales and the Counts and Deeg-Paterson[8] scales were found to be .98 and .97, respectively. These values indicate a remarkable inter-generational stability in occupational status valuations in this society.

CHARACTERISTICS OF SAMPLE COMPONENTS

Comparison of mail and interview returns in the Northeast disclosed a number of differences between the two. The mail group was approximately three years younger than the interview group—a difference which was not statistically significant—and had a mean occupational origin 2.8 North-Hatt points higher. The latter difference was significant at the .01 level. The mail group also showed a positive regression of origins on time, while the interview group showed negative regression. Neither regression trend differed significantly from zero, and the difference between the two trends was estimated to be non-significant, judging by the magnitudes of the standard errors of the regression coefficients.

Despite the significantly lower origin of the interview group in the Northeast, the interview and mail returns were combined for analysis. Lack of interview returns for the Midwest precluded construction of an approximately random working sample for both regions. Consequently, the interview returns from the Northeast were used primarily as a clue to the direction and magnitude of error in the mail returns. This provided some guard against gross error in interpretation, and also permitted rough adjustments for sampling bias.

Although no interview returns were obtained in the Midwest, it was concluded that the means of mail and interview samples would have differed less in that region than in the Northeast. This inference was based upon comparisons between mail and interview returns in an earlier study of physicians.[9] In that study the two types of return diverged far more in the Northeast than in the Midwest; also, the divergence of the physician samples in the Northeast exceeded considerably that of the present samples of business leaders.

INTER-CITY AND INTER-REGIONAL DIFFERENCES IN ORIGIN

The first point of interest in this study was the possibility of a significant difference in origin means between the Northeast and Midwest regions. The presence of such a difference would be a matter of considerable theoretical importance. First, it would permit deductions regarding historical trends in occupational origins, since conditions in the older region could ordinarily be assumed to have chronological priority. Second, variations in origin between regions would allow some inferences concerning the relation of social and economic differences to the observed differences in occupational mobility.

In reality, differences in origin between regions proved slight. For mail returns alone, the four cities from east to west

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[5] Cecil C. North and Paul K. Hatt, "National Opinion on Occupations," *Opinion News,* 9 (September, 1947), pp. 3–13.

[6] Leon Festinger and Daniel Katz, *Research Methods in the Behavioral Sciences,* New York: Dryden Press, 1953, Ch. 11.

[7] G. S. Counts "Social Status of Occupations: A Problem in Vocational Guidance," *School Review,* 33 (1925), pp. 16–27.

[8] M. E. Deeg and D. G. Paterson, "Changes in Social Status of Occupations," *Occupations,* 25 (1947), pp. 205–208.

[9] Stuart Adams, "Trends in Occupational Origins of Physicians," *American Sociological Review,* 18 (August, 1953), pp. 404–409.

yielded origin means of approximately 79, 78, 78, and 78 North-Hatt points, respectively. In Table 1, which combines mail and interview returns for the Northeast, the corresponding means are 77, 77, 78, and 78, respectively. If the Midwest cities also had produced interview returns, it is estimated that the final means would have approximated 77 North-Hatt point in every instance.

TABLE 1. OCCUPATIONAL ORIGIN MEANS OF BUSINESS LEADERS BY CITY AND REGION

Locality	Number of Informants	Mean North-Hatt Score	Standard Deviation
Worcester	62	77.37	6.93
Springfield	56	77.29	7.49
Columbus	52	78.03	6.45
Toledo	50	78.36	7.33
Northeast	118	77.33	6.55
Midwest	102	78.19	6.70
Total	220	77.73	6.91

It is evident from Table 1 that there are no significant inter-city or inter-regional differences in mean occupational origins of the respondents. This finding contrasts markedly with results obtained for attorneys and physicians in the same four cities.[10] In the two latter occupations, origins were significantly lower in the Northeast, suggesting that there were, or had been at some earlier time, higher rates of mobility into the professions in this region than in the Midwest.

For the business leaders in the present study, mobility into the group appears to be uniform in rate across the two regions. By the preceding logic this would indicate that either (1) there has been no appreciable change in rates of vertical mobility into positions of business leadership in recent decades, or (2) factors conducive to changes in mobility have been distributed so as to equalize rates between regions.

SECULAR TREND IN OCCUPATIONAL ORIGINS

One view of the secular trend in business leader origins is provided by the mean occupational origins of specific age groups of the respondents. This view is not the broad perspective on mobility rates which is presumably contained in the regional means. However, it gives a clearer indication of short-term fluctuations in rate. Table 2 shows the means trend for the total sample of 220 respondents.

Several points are worthy of note in Table 2. One is the general though non-significant decline in origin means with time, excluding the decade 1900 to 1910. A second is the pronounced rise in origins in the atypical decade—a rise which is significant at the .05 level in relation to the overall mean. A third is the large drop in standard deviation for that decade relative to the decades preceding and following.

TABLE 2. MEAN OCCUPATIONAL ORIGIN OF BUSINESS LEADERS BY DECADE OF BIRTH

Decade of Birth	Number of Informants	Mean North-Hatt Score	Standard Deviation
1871–1880	14	77.8	4.50
1881–1890	53	77.9	4.96
1891–1900	78	77.0	8.02
1901–1910	45	79.4	4.53
1911–1920	30	76.8	8.17
Total	220	77.7	6.91

These data, though presently limited in statistical significance, appear to support the following conclusions. First, there was a general trend toward lower occupational origins among the respondents during the period under study here. There was also a definite increase in mean origin among respondents entering the channels to business leadership during the period 1930 to 1940, roughly, the years of the last great depression. This increase in origins resulted primarily from a contraction of the recruiting range of prospective leaders, not from a general upward shift of the range.

It also appears that there was a progressive broadening of the base from which prospective leaders were being drawn during the past fifty years, again excluding the atypical decade. This seems to be the meaning of the continual rise in the standard deviation of the occupational origin means. Judging by the trend in the origin

[10] Adams, *op. cit.*; also, "Regional Differences in Vertical Mobility in a High-Status Occupation," *American Sociological Review*, 15 (April, 1950), pp. 228–235.

means, the apparent broadening occurred by the inclusion of individuals from progressively lower strata, not from higher strata previously unrepresented in this occupational group.

An alternative view of secular trends in origins of the subject group is available in the regression coefficients of the two variables in the city and regional samples. Table 3 shows the regression of occupational origins on time for the major subdivisions of the sample.

TABLE 3. REGRESSION OF OCCUPATIONAL ORIGIN MEANS ON TIME

Locality	Number of Informants	Coefficient of Regression	Standard Error
Worcester	61	.07	.07
Springfield	57	—.01	.09
Columbus	52	—.06	.09
Toledo	50	.01	.10
Northeast	118	.03	.06
Midwest	102	—.03	.06

The data in Table 3 suggest that there may be some variation in regression trends in the various cities and regions. In view of the size of the standard errors, however, it is clear that none of the coefficients of regression are significant. Like the means trend in Table 2, these coefficients appear to indicate that there has been no real tendency for occupational origins of respondents to move either upward or downward over the last two generations.

It is relevant to consider at this point the effect of two kinds of adjustments in the data. One is the omission of respondents born in the decade 1900 to 1910, i.e., a correction for the presumed effects of the depression of the nineteen-thirties. The other is the weighting of the interview returns by three, a compensation for failure to interview all non-respondents to the Northeast cities. One consequence of the two adjustments is a shift in regression coefficient for the total sample from approximately zero to —.05. At the same time the probability of the coefficient shifts from near unity to .20. For the Northeast, the corresponding values are —.07 and .15. These coefficients also fail to reach significance, but they strongly suggest that a random sample of informants, controlled for incidents such as depressions, is very likely to show a downward trend in origins with time.

BUSINESS SIZE AND OCCUPATIONAL ORIGINS

The relationship between business size and occupational origin was studied by plotting occupational origin scores against numbers of persons employed. The means trend thus obtained was apparently curvilinear, with the highest origins occurring among leaders of "middle-sized" businesses. The mean origins of the leaders of six arbitrarily designated [11] size groups are shown in Table 4.

The scatter plot for the two variables shows a decided constriction in the zone of intermediate numbers of employees. There is, conversely, a wide dispersion toward both ends of the size scale. With respect to differences in origin, the mean of Group IV is significantly higher than the mean for the total group. Groups I and VI, while lower than the total group, are not significantly lower. These two groups are significantly lower in mean origin, however, than Group IV.

[11] The groups were chosen to include roughly comparable areas symmetrically placed around the high-origins group, with at least two groups on each side of the high group.

TABLE 4. OCCUPATIONAL ORIGIN MEANS OF BUSINESS LEADERS BY NUMBER OF EMPLOYEES

Group	Number of Employees	Number of Informants	Mean North-Hatt Score	Standard Deviation
I	100–149	72	76.4	7.5
II	150–299	47	77.0	7.9
III	300–499	38	77.7	6.0
IV	500–999	30	82.1	4.6
V	1000–4999	26	79.1	6.1
VI	5000–30000	7	72.8	10.9
	Total	220	77.7	6.91

The configuration of means in Table 4 suggests that there are important differences in leader selection, quantitatively and possibly qualitatively, between the ends and center of the business-size range. In particular, Group IV appears to be more exclusive in its recruiting procedures than any other group in the distribution. Among the apparent explanations of this difference, the higher percentage of owner-managed businesses in Group IV may deserve mention. The only group having a higher percentage of such businesses is Group I. This pattern of ownership and origin means suggests that traditionalistic criteria may operate more consistently in the selection of leaders in the intermediate group than in the remaining groups.

The data in Table 4 are meaningful for trends in occupational origin primarily in relation to economic processes in this society. To clarify this meaning, it seems particularly important to note the significance of changes in the following: (1) the distribution of rational leader-selection procedures in the economic organization, (2) the proportion of the economy contained in businesses in the middle-sized category, and (3) the extent of corporate ownership of business.

Changes in these factors have some rather obvious consequences for trends in origin means. If the objective selection procedures now found in many large concerns should diffuse, the proportion of business leaders who will have been screened by rationalistic criteria will rise. Furthermore, if there is some tendency for businesses to shift from units of intermediate to large size, and from family to corporate ownership, a corresponding reduction of leaders in high-origin groups is implied.

The practical importance of these tendencies is a function of their actual existence in economic affairs; hence, their status in thought and action is a matter of considerable interest. Much notice has been given to the growing movement for leader selection, which ostensibly emphasizes the functional requirements of the leader role over considerations of lineage or personal interest.[12] An important part of the movement is the recruitment of exceptional college graduates for apprenticeship in the middle levels of management.[13] This development is of special interest here because it appears to give education increased importance in succession to top levels of management. In this way, large business seems to be establishing accessibility to education as a major criterion of entrance into its leader group, suggesting that executives may come to show increasingly some of the mobility patterns now found in the high-status professions.

With regard to the remaining factors, the tendency for large businesses to absorb or displace smaller units has been well noted.[14] The existence of management problems peculiar to middle-sized businesses and operating as a factor in their relatively high vulnerability has also received attention.[15] Most conspicuous of all, of course, is the tendency toward dispersion of ownership of business in the contemporary American economy.

The extent to which the above tendencies are rising, stabilized or declining trends is open to some debate. If they have entered a declining phase, their ability to accelerate executive mobility will have diminished accordingly. As a summary judgment, however, it may be suggested that some of these tendencies are currently operating to increase the rationality of leader selection, or to reduce the number of leader positions in which traditionalistic criteria are most likely to exert heavy influence.

DISCUSSION

Two important conclusions appear to be supported by the foregoing analysis. The first is that there was an apparent decline in occupational origins among the informants

[12] "The Tests of Management," *Fortune*, 42 (July, 1950), pp. 92 ff.

[13] "The Crown Princes of Business," *Fortune*, 48 (October, 1953), pp. 150 ff.; Peter Drucker, *The New Society*, New York: Harper and Bros., 1949, p. 221.

[14] Temporary National Economic Committee, *Final Report and Recommendations*, Washington: U. S. Government Printing Office, 1941; Federal Trade Commission, *The Concentration of Productive Facilities*, Washington: U. S. Government Printing Office, 1949; John K. Galbraith, *American Capitalism*, Boston: Houghton Mifflin Co., 1952.

[15] Peter Drucker, *op. cit.*, pp. 226–227; Burleigh Gardner, "Ability Held Need in Small Business," *New York Times*, April 24, 1949.

in this study over the span of time covered by the data. The limited statistical significance of the unadjusted data requires that this conclusion be taken with some caution, particularly until the validity of the suggested adjustments can be ascertained.

The second, and perhaps more significant conclusion is that there was a marked rise in occupational origins of the informants during one decade of the observed period, presumably as a result of the depression, and an equally marked decline in the years following. This pattern of events appears to signify that there was a hardening of the business occupational structure during this period, and, also, that the hardening was a transitory phenomenon. Once the effects of the depression were dissipated, the trend in origins apparently resumed its downward direction. The importance of the second conclusion is that it provides one explanation of the current controversy over the hardening of class lines in this society. The hyphothesis of increasing occupational inheritance in the prestige occupations was apparently confirmed by the data of the nineteen-thirties.[16] Yet this confirmation, it would appear from the present data, was clearly limited in time. On the basis of these findings, some of the temporal generalizations from the mobility or class-structure studies of the last two decades are no longer tenable.

While the constriction of the recruiting range of business leaders in the years 1930 to 1940 was a unique phenomenon temporally, it was a commonplace occurrence occupationally. The legal profession, particularly, showed a strong contraction of recruitment range among prospective attorneys born in the early years of the present century. There is also evidence of this tendency in the data on physicians, although medicine appears to have shown greater stability in its recruiting practices during the depression than either law or big business.[17]

In interpreting the foregoing means and regression coefficients, it is important to note two characteristics of the origin indices. One of these is the probability that the more recent means underrepresent business leaders of relatively low origins because of their longer apprenticeship in preparatory roles. It is a logical assumption that individuals who start without a lineage in business may have to spend a portion of their careers compensating for this deficiency. A parallel case is the tendency for individuals from low status families to take longer to complete their professional training because of reduced semester hours or because of occasional interludes of employment. The time lag in the "arrival" of business leaders cannot be estimated from the present data. The effect of such lag, however, would be to shift the regression of origin means in a positive direction, thus aiding in the appearance of an increase in occupational inheritance.

A factor with similar consequences for origin means is the gradual shift toward higher occupational statuses in this society. This shift is occasioned by redistributions in the occupational profile, including the shrinkage of relative numbers in some low-status occupations and an expansion in some high-status occupations. Given a rising mean occupational status for the total population, constancy in the mean status origins of a particular occupational group signifies an actual decline relative to the base from which the recruits were drawn. Consequently the current means trend for business leaders as measured here is again unduly weighted in the positive, i.e., "increasing inheritance" direction.

Although a precise adjustment for rising status of the working population is not readily available, rough approximations are possible. Applying North-Hatt scores to the Census profiles of workers for 1910 and 1950 yields mean scores of 59.5 and 63.0, respectively. This suggests a correction of 3.5 points in the origin-mean differential between older and younger informants in the present sample, assuming that business leaders are drawn randomly from the working population. This is obviously not the case, but the error in this assumption does not appear to require a correction less than the one proposed. It may be reasoned, therefore, that the origin means of business leaders in this sample, apart from adjustments for the depression period and for underrepresen-

[16] The "compelling" character of the data of the nineteen-thirties is strikingly evident in the contrasting interpretations of mobility chances offered by the Lynds in *Middletown* and *Middletown in Transition*.

[17] Adams, *op. cit.*

tation of the interview group, should be approximately 78, 77, 75, 77 and 73, respectively, for the five decades studied. The decline in origin means in this series would be significant at the .01 level.

SUMMARY AND CONCLUSION

To provide information on the general correlates of occupational origins of business leaders, data were obtained from samples of business heads in four cities located in two ecological regions. Mailed questionnaires and follow-up interviews with random samples of non-respondents were employed as field techniques. The follow-up interviews, which were intended as a check on bias in the mail returns as well as a means of increasing the number of respondents, were restricted to two of the four cities.

A total of 178 mail returns and 42 interview returns were obtained from 510 prospective informants. Analysis of the occupational origins of these informants indicated the following:

(1) Occupational origin means of the respondents did not differ significantly between cities or between regions.

(2) There was no clear-cut tendency in the unadjusted data for origins either to increase or decrease in time. If adjustments were made for (a) restriction in range of recruitment during one decade, (b) a significant bias in the mail returns, and (c) the secular trend in mean occupational status of the American working population, the origin means showed a highly significant downward trend with time.

(3) The relationship between occupational origin and size of business appeared to be curvilinear. Highest origins were found among business leaders in middle-sized businesses, and the lowest origins among leaders of the largest businesses. The mean origin of the latter group was significantly lower than in the group with intermediate numbers of employees. This pattern of origin means suggests that traditionalistic selection criteria are relatively strong in the middle-sized businesses, while rationalistic criteria are relatively strong in the larger and smaller businesses. Viewed in relation to current economic trends, this means configuration appears to imply an increase in the use of achieved statuses as leader selection criteria in the future.

(4) A marked constriction of the recruiting range of respondents born in the years 1900 to 1910 suggests that the depression had a significant but transient stratifying effect upon this occupational group. The perception of this effect and its projection into the future may have been the source of several erroneous predictions regarding contemporary trends in vertical mobility in this society.

DIFFERENTIAL FERTILITY IN THE UNITED STATES: 1900 TO 1952*

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THIS paper is addressed to the specific question of whether there has been an expansion or contraction of group differences in fertility in the United States since 1900. Some aspects of this problem have been investigated in varying detail for the United States,[1] Great Britain, and other countries in Western Europe.[2] The general consensus of these studies is that urban-rural, occupational, and other group differences in fertility widened in the latter half of the nineteenth century, presumably as a result of an uneven spread and acceptance of contraceptive knowledge and practices. Developments since the turn of the century, however, have been more complex. Studies within the past twenty years or so in the United States have repeatedly reported evidence of exceptions to the general rule of an inverse relation of fertility to socio-economic status,[3] that is, fertility in the middle groups being the same or lower than that of the higher classes. This has been interpreted by many as a consequence of the democratization of birth control usage and as the foreshadowing of a general contraction of group fertility differences. The hypothesis adduced to account for this development prior to 1940 is that this contraction was primarily a function of a more rapid rate of *decrease* in the fertility of the lower socio-economic groups. Since 1940, the contractions that have been observed are accredited to higher rates of *increase* among the higher socio-economic groups.[4]

The hypothesis that differences in fertility between urban and rural areas in the United States have been in the process of contracting has been seriously questioned by

* A summary of this paper was prepared for presentation at the World Population Conference of the International Union for the Scientific Study of Population meeting in Rome, Italy, August 31 to September 10, 1954.

[1] See, e.g., Clyde V. Kiser, "Fertility Trends and Differentials in the United States," *Journal of the American Statistical Association,* 47, No. 257 (March, 1952), pp. 25–48, especially pp. 37–48; Robert M. Dinkel, "Occupation and Fertility in the United States," *American Sociological Review,* 17, No. 2 (April, 1952), pp. 178–183; Evelyn M. Kitagawa, "Differential Fertility in Chicago, 1920–1940," *American Journal of Sociology,* LVIII, No. 5 (March, 1953), pp. 481–492; T. J. Woofter, "Trends in Rural and Urban Fertility Rates," *Rural Sociology,* 13, No. 1 (March, 1948), pp. 3-9; X. Sallume and Frank W. Notestein, "Trends in the Size of Families Completed Prior to 1910 in Various Social Classes," *American Journal of Sociology,* 33, No. 3 (November, 1932), pp. 398–408; Clyde V. Kiser, "Trends in the Fertility of Social Classes from 1900 to 1910," *Human Biology,* 5, No. 2 (May, 1933), pp. 256–273; Frank Lorimer and Frederick Osborn, *Dynamics of Population,* New York: The Macmillan Co., 1934.

[2] John W. Innes, *Class Fertility Trends in England and Wales,* Princeton: Princeton University Press, 1938; David V. Glass and E. Grebenik, "The Family Census: A Preliminary Report," in *Papers of the Royal Commission on Population,* II, London, 1950; Dennis Wrong, *Trends in Class Fertility Differentials in Western Nations,* Ph.D. thesis, Columbia University (in preparation).

For other references to American and British studies, see bibliography in Charles F. Westoff, "The Changing Focus of Differential Fertility Research: The Social Mobility Hypothesis," The Milbank Memorial Fund *Quarterly,* January, 1953, xxxl, No. 1, pp. 24–38. For a more extensive list of international references and summary, see United Nations, Population Commission: *The Determinants and Consequences of Population Trends,* New York: United Nations Publication, 1953 (especially Chap. V and bibliography).

[3] See e.g., Frank W. Notestein, "Differential Fertility in the East North Central States," The Milbank Memorial Fund *Quarterly,* xvi, No. 2 (April, 1938), pp. 186–191; Clyde V. Kiser, *Group Differences in Urban Fertility,* Baltimore: The Williams and Wilkins Company, 1942; Clyde V. Kiser and P. K. Whelpton, Social and Psychological Factors Affecting Fertility IX. "Fertility Planning and Fertility Rates by Socio-Economic Status," The Milbank Memorial Fund *Quarterly,* xxvii, No. 2 (April, 1949), pp. 188–244.

[4] Clyde V. Kiser, "Fertility Trends and Differentials in the United States," p. 45. In this article, Kiser's attention is focused on comparisons of 1940, 1947, and 1949 data.

Woofter,[5] who reported in his analysis that "the data as they stand are . . . at odds with preconceived ideas . . ." and that "available measures of fertility rates from 1910 to 1940 indicate some *widening** of the gap between birth rates on the farm and in the city . . . ." Woofter's net conclusion is that the effect of rural-urban migration on these fertility measures is so problematical that no final answers can be given.

It becomes quite clear to anyone reviewing the literature on this subject that very few unequivocal generalizations about trends in differential fertility relationships in the United States can be drawn. This present paper is an attempt to shed more light on the subject by assembling more comparative data on several main variables from 1900 to 1950 and incorporating some recent findings of a 1952 national sample survey.[6] Because of the nature of the data, most of the usual methodological problems remain unsolved; in some of the comparisons attempted, additional methodological impurities are introduced. The methodological objections of Woofter and others still obtain.

THE COLOR AND URBAN-RURAL DIFFERENTIAL

In the absence of complete birth data extending back to the turn of the century, the decennial census enumeration of children under 5 years of age can be used as a rough measure of births. The ratio of children under 5 to women of reproductive age (20–44) is used here as an estimate of the fertility of these women for the five years prior to each census. The numerous inadequacies of this measure have been elaborated by others.[7] Three specifically related problems involve the well-known underenumeration of children under 5, the mortality of these children, and the mortality of women aged 20 to 44. Two other problems, not confined to this measure, are differences in the age distribution of these women over time and among population subgroups at a given time, and the concealed effects on fertility of changes in the marriage rate, age at marriage, and duration of marriage. A crude solution of some of these difficulties has been attempted here. In subsequent sections of this paper, other types of fertility measures have been used which are not wholly susceptible to all of these drawbacks. For the present discussion of color and urban-rural fertility differentials, however, the simple age distributions presented in the respective census volumes have been used to obtain the fertility ratios. These ratios have been adjusted here both for underenumeration and mortality of children under 5 [8] and have been standardized for age of women.[9]

The fertility ratios by color and urban-rural residence for the last half century are presented in Table 1 and Figures 1 and 2. The general trend for all groups between 1900 or 1910 and 1940 is downward; from

[5] *Op. cit.*

* Italics mine.

[6] U. S. Bureau of the Census: *Current Population Reports—Population Characteristics,* Series P. 20, No. 46, December 31, 1953. The author would like to take this opportunity to express appreciation for the efforts that the Census Bureau has made to secure these sample data on fertility in the past and to express the hope that this work will continue in the future.

[7] For excellent discussions see Everett S. and Anne S. Lee, "The Differential Fertility of the American Negro," *American Sociological Review,* 17 (August, 1952), pp. 437–439; and Warren S. Thompson, *Ratio of Children to Women, 1920,* Census Monograph XI, Washington, 1931, pp. 15–17.

[8] The underenumeration and mortality corrections were accomplished simultaneously for white and nonwhite populations separately. The technique used for each census was to assume that the enumeration of children 10 to 14 years old is an accurate estimate of the number of children born 10 to 14 years earlier minus the mortality experienced by this birth cohort. The observed number of children 10 to 14 years of age was then inflated by the appropriate life table values giving (again for white and nonwhite groups separately) the proportion of children dying between ages 10 and 15 (100 minus the average of the 1_x values for ages 10 and 15). The original enumerated number of children under 5 was then compared with the resultant computation and a correction factor derived. Urban and rural corrections, although desirable, could not be obtained because of internal migration.

[9] Because the data available in the conventional census reports do not give the distribution of number of children under 5 by age of woman, the method of indirect standardization was necessary. The standard population used was that of women by age and by number of children under 5 in 1940. This standard was computed from data in a special monograph series—U. S. Bureau of the Census: *Sixteenth Census of the United States, Population, Differential Fertility, 1940, and 1910, Women by Number of Children Under 5 Years Old,* Washington, 1945, p. 11.

TABLE 1. NUMBER OF CHILDREN UNDER 5 PER 1,000 WOMEN 20–44 YEARS OLD, FOR THE UNITED STATES BY COLOR AND URBAN-RURAL RESIDENCE: 1900 TO 1950 (ALL RATIOS STANDARDIZED FOR AGE OF WOMEN AND CORRECTED FOR UNDERENUMERATION AND MORTALITY OF CHILDREN)

Color and Residence	Fertility Ratios							Per Cent of Change				
	1950											
	New Urban Definition	Old[1] Urban Definition	1940	1930[2]	1920	1910[3]	1900[4]	1940–1950[5]	1930–1940	1920–1930	1910–1920	1900–1910
All classes	622		466	571	675	748	812	+33	−18	−15	−10	−8
All classes:												
White	608		454	552	665	721	768	+34	−18	−17	−8	−6
Nonwhite	740		565	623	717	953	1,141	+31	−9	−13	−25	−16
All classes:												
Urban	545	534	354	436	524	551	...	+51	−19	−17	−5	...
Rural nonfarm	747	729	559	671	760	...	...	+30	−17	−12	...	...
Rural farm	876	875	752	865	953	...	...	+16	−13	−9	...	...
(Total rural)	795	775	650	771	872	976	...	+19	−16	−12	−11	...
White:												
Urban	540	529	354	440	536	557	...	+49	−20	−18	−4	...
Rural nonfarm	728	712	553	670	764	...	...	+29	−17	−12	...	...
Rural farm	812	811	709	837	927	...	...	+14	−15	−10	...	...
(Total rural)	758	742	623	752	855	928	...	+19	−17	−12	−8	...
Nonwhite:												
Urban	584	576	360	397	387	475	...	+60	−9	+3	−19	...
Rural nonfarm	962	929	619	684	719	...	...	+50	−10	−5	...	...
Rural farm	1,266	1,265	966	996	1,082	...	...	+31	−3	−8	...	...
(Total rural)	1,115	1,083	837	886	970	1,241	...	+29	−6	−9	−22	...

[1] Computed from Census Bureau estimates of the age distribution of the 1950 population according to 1940 Census definitions of urban-rural residence. The Census Bureau estimated the age distribution for the rural nonfarm and rural farm populations. Statistics for the urban population and for total children under 5 for all three classes were not estimated. Comparison of the above ratios for the two definitions suggests that there may have been an over-estimate of the number of rural nonfarm women 20 to 44 years old, implying that the actual fertility ratio should be somewhat higher for this group according to the 1940 definition. The main consideration is the age distribution of women living in suburban areas, since this group was directly affected in the re-definition.

[2] Mexicans classified as white.

[3] The subdivision of rural into nonfarm and farm residence in 1910 is not presented for these fertility ratios for various reasons of noncomparability with later census statistics. This detail for 1910 is shown for other types of fertility measures which include comparisons based wholly on special Census Monograph Series.

[4] The Census of 1900 did not employ the urban-rural definitions used in later censuses.

[5] Per cent of change computed on basis of the 1940 Census definition of urban-rural residence.

1940 to 1950 each group experienced substantial increases in its fertility. Kiser's generalization for the 1940–1947 period that the "increases in fertility ratios since 1940 have tended to be proportionately heaviest among groups previously characterized by lowest fertility and lightest among groups of highest fertility" [10] is essentially accurate for the 1940–1950 comparisons presented here. The one apparent exception to this observation is the slightly greater increase in nonwhite than in white fertility ratios in rural areas.

Although all of these sub-groups of the population contributed in varying measure to the baby boom, the trend in completed size of families was still downward in 1952. The average number of children ever born to women ever married who have passed through their reproductive age was about 5.1 children in 1910, 3.3 in 1940, and 2.8

[10] "Fertility Trends and Differentials in the United States," p. 38.

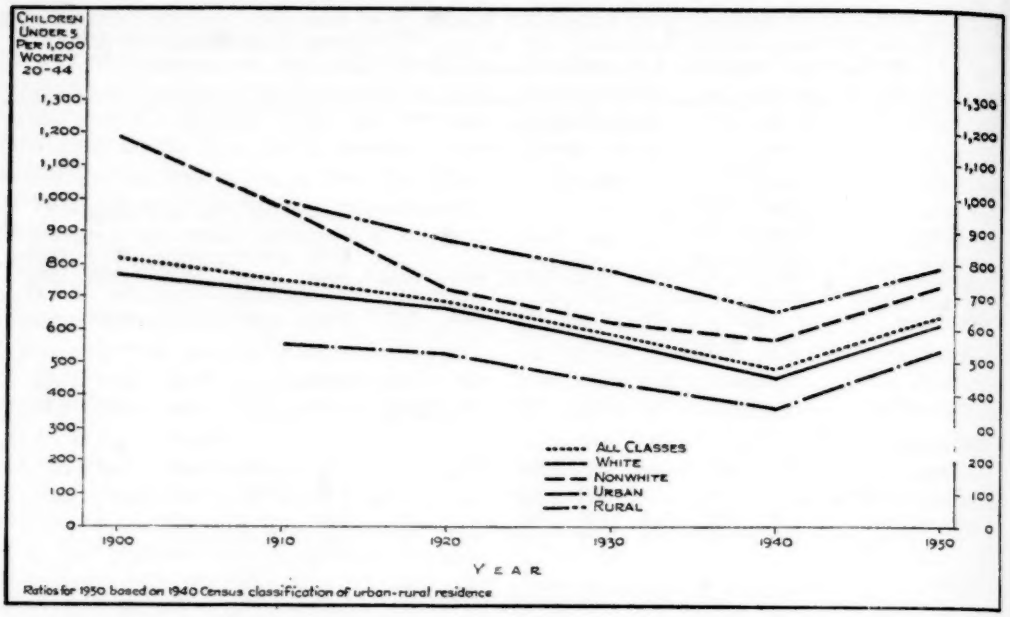

FIGURE 1. Number of Children under 5 per 1,000 Women 20–44 Years Old for the United States by Color, 1900 to 1950, and by Urban-Rural Residence, 1910 to 1950. (Ratios standardized and adjusted; see Table 1 for complete description.)

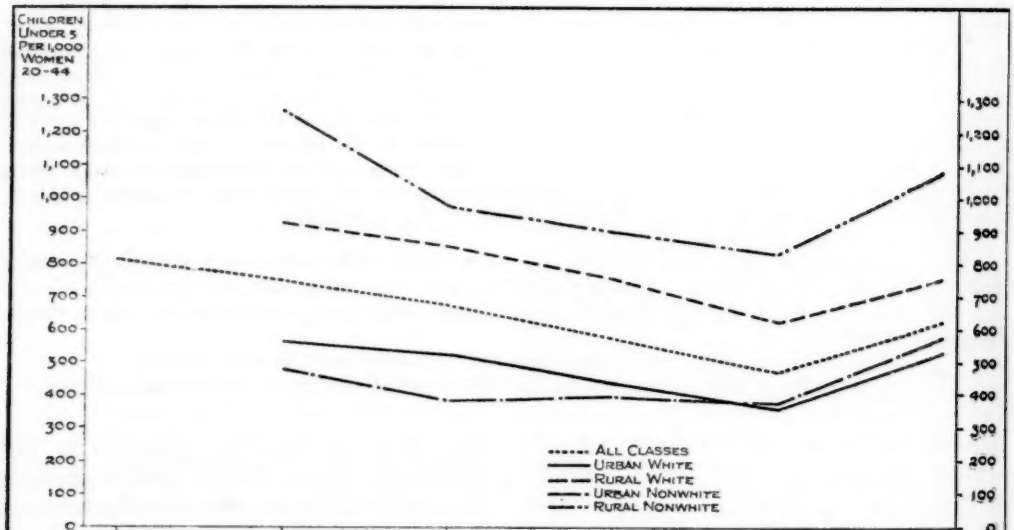

FIGURE 2. Number of Children under 5 per 1,0000 Women 20–44 Years Old for the United States, 1900 to 1950, and by Color Cross-Classified with Urban-Rural Residence 1910 to 1950. (Ratios standardized and adjusted; see Table 1 for complete description.)

in 1952 for the total population[11] (see Table 2).

The relationships between white and nonwhite, and urban and rural, fertility over the past fifty years are analyzed in Table 3 by the use of ratios of white to nonwhite fertility, and urban to rural fertility, for both current and completed fertility. No single generalization of trends in these relationships can be made. The color differential (current fertility) seems to have narrowed up to 1920 to .93 and then to have leveled off between .80 and .90. The ratios for completed size of family suggest a narrowing of the color differential. White and nonwhite fertility are closer for the urban (.92 in 1950) than they are for the rural population (.69 in 1950). This is true as well for completed fertility, which showed a ratio of .96 in 1940 for the urban population and .80 for the rural population.

TABLE 2. COMPLETED FERTILITY[1] OF WOMEN EVER MARRIED FOR THE UNITED STATES BY COLOR AND URBAN-RURAL RESIDENCE: 1952,[2] 1940, AND 1910

Color and Residence	Per 1,000 Women	Children Ever Born					
		Total	None	1	2	3–4	5 or More
				1952			
All classes	2,813	100.0	17.6	18.5	20.5	23.0	20.3
Whites	2,775	100.0	17.1	18.9	20.9	23.4	19.7
Nonwhites	3,246	100.0	23.1	14.6	15.6	18.8	27.8
Urban	2,571	100.0	18.4	19.9	21.8	23.2	16.7
Rural nonfarm	3,089	100.0	17.9	16.7	18.7	21.5	25.2
Rural farm	3,645	100.0	13.2	14.3	16.7	24.3	31.6
(Total rural)	3,326	100.0	15.9	15.7	17.8	22.7	27.9
				1940			
All classes	3,346	100.0	14.8	14.8	17.5	25.3	27.6
Whites	3,301	100.1	14.7	15.0	17.8	25.6	27.0
Nonwhites	3,891	100.1	17.1	12.6	12.9	21.7	35.8
Urban	2,912	100.0	17.1	16.7	19.3	25.5	21.4
Rural nonfarm	3,558	99.9	13.8	13.9	16.3	25.3	30.6
Rural farm	4,449	100.0	8.9	9.8	13.1	25.1	43.1
(Total rural)	4,018	100.1	11.3	11.8	14.7	25.2	37.1
				1910			
All classes	5,065	100.0	8.7	9.1	10.9	21.0	50.3
Whites	4,924	100.0	8.8	9.1	11.2	21.6	49.3
Nonwhites	6,613	100.0	7.5	8.4	7.9	14.7	61.5
Urban	4,526	100.1	10.2	10.7	12.5	22.6	44.1
Rural nonfarm	5,001	100.1	9.0	9.2	11.1	21.4	49.4
Rural farm	5,988	100.0	6.0	6.4	8.2	18.3	61.1
(Total rural)	5,571	100.0	7.2	7.6	9.4	19.6	56.2

[1] Statistics for 1952 based on women 45 years old and over; data for 1940 and 1910 based on women 45–74 years of age.

[2] Statistics for 1952 include adjustment for the estimated fertility of women not reporting on number of children ever born. Residence classification for 1952 is according to the new definition of urban areas.

Source: Statistics for 1952 adapted from U. S. Bureau of the Census: *Current Population Reports—Population Characteristics*, Series P-20, No. 46 (December 31, 1953), pp. 12–14. Statistics for 1940 and 1910 computed from data in U. S. Bureau of the Census; *Sixteenth Census of the United States. Population: Differential Fertility 1940 and 1910: Fertility for States and Large Cities*, Washington: Government Printing Office, 1943, pp. 7–17.

[11] The data for 1952 are based on results of the sample from the Current Population Survey. The data for 1940 and 1910 are derived from sample studies of the 1940 and 1910 Censuses. The nature of the respective samples is discussed adequately in the preliminary texts in these publications (*cf.* sources listed in table footnotes).

The urban-rural fertility differential appears to have been fairly stable between 1910 and 1940, ranging between .54 and .60. Owing to the greater increase in uban fertility between 1940 and 1950, this ratio increased to .69 in 1950. The evidence from completed fertility is inconclusive. For current fertility of nonwhites, there is some suggestion of a contraction of the differential, although even by 1950 the rate for rural nonwhites a decade behind that of whites (and this only in rural areas).[12] Along this line, it is pertinent to note that the color differential as measured by proportions childless has been reversed since 1910. In that year, among white women 45 years of age and over, about 9 per cent reported having had no children; for nonwhite women this figure was 7.5 per cent. By 1940 at least (probably much earlier), this relationship had been reversed

TABLE 3. DIFFERENTIAL FERTILITY RATIOS FOR THE UNITED STATES BY COLOR AND URBAN-RURAL RESIDENCE FOR CURRENT FERTILITY 1900 TO 1950 AND FOR COMPLETED FERTILITY 1952, 1940, AND 1910

Year	Whites to Nonwhites	Urban to Rural	Urban: Whites to Nonwhites	Rural: Whites to Nonwhites	Whites: Urban to Rural	Nonwhites: Urban to Rural
			Current Fertility			
1950	.82	.69	.92	.69	.71	.53
1940	.80	.54	.98	.74	.57	.43
1930	.89	.57	1.11	.85	.59	.45
1920	.93	.60	1.39	.88	.63	.40
1910	.76	.56	1.17	.75	.60	.38
1900	.67	*	*	*	*	*
			Completed Fertility			
1952	.86	.77	*	*	*	*
1940	.85	.73	.96	.80	.74	.62
1910	.75	.81	.87	.73	.84	.71

* Base data not available.
Source: Computed largely from statistics presented in Tables 1 and 2.

was about twice as high as that for urban nonwhites.

In general, the color differential offers more evidence in support of the hypothesis that fertility differentials are narrowing; analysis of the urban-rural differential proves inconclusive except among nonwhites where it appears to be very slowly narrowing. The difference between the fertility of urban and rural areas remains greater within white and nonwhite classifications than the difference between the fertility of the white and nonwhite populations with residence held constant. In short, residence in the city or in the country is related to greater differences in fertility than is membership in the white or nonwhite color group. This is due largely to the automatic separation of occupation (with its implied differences in values and styles of life) in the urban-rural classification. It is tempting to argue from the data in Table 1 that the fertility behavior of the nonwhite population is lagging only about and in 1952 there was an incidence of childlessness of 17 per cent for whites as compared to 23 per cent for nonwhites.

THE EDUCATIONAL AND OCCUPATIONAL DIFFERENTIAL

Unfortunately, there are no data on education by fertility currently available[13] that would reflect the more recent increase in births that has occurred in and since 1947. The data in Table 4 and Figure 3 reflect fertility experience between 1942 and 1947 and between 1935 and 1940. The fertility ratios for both of these periods show an inverse relationship with women's education. The rates of change between these two

[12] For additional evidence of similarities in reproductive behavior, see "The Differential Fertility of the American Negro."

[13] This information and other data on fertility from the 1950 Census will be available in tabulation form in the near future.

TABLE 4. NUMBER OF CHILDREN UNDER 5 PER 1,000 WOMEN 15 TO 49 YEARS OLD, STANDARDIZED FOR AGE OF WOMAN,[1] BY YEARS OF SCHOOL COMPLETED AND MARITAL STATUS OF WOMAN, FOR THE UNITED STATES.[2]

Years of School Completed	All Marital Classes			Married, Husband Present		
	1947	1940	Per Cent of Change	1947	1940[3]	Per Cent of Change
Total	367	284	29	526	465	13
Grade school:						
Less than 5 years	508	405	25	718	634	13
5 and 6 years	477	405	18	661	589	12
7 and 8 years	422	339	24	572	505	13
High school:						
1 to 3 years	396	292	36	535	444	20
4 years	323	218	48	465	381	22
College:						
1 to 3 years	306	197	55	482	368	31
4 years or more	271	153	77	446	333	34

[1] Ratios for all marital classes standardized by the age distribution of all civilian women in the United States in 1947; ratios for women married with husband present standardized by the age distribution of this class in the 1947 civilian population.

[2] Ratios for 1947 refer to the civilian population; ratios for 1940 refer to the total population. The ratios for 1940 are estimates from data for native-white and Negro women who in 1940 comprised about 93 per cent of all women 15 to 49 years old.

[3] Married once, husband present.

Source: U. S. Bureau of the Census, *Current Population Reports—Population Characteristics*, Series P-20, No. 18 (June 30, 1948), p. 6.

periods indicate that the higher educational classes contributed proportionately more births to the 1942–1947 increase than women of lower educational attainment. The increase for college graduates, for example, was close to three times as great as the increase for women with less than 5 years of grade school education. The effect of this differential increase has been to narrow the differences among the various educational classes. For 1940, the average percentage difference between the marital fertility rates of each class and the unweighted average of the seven is 21 per cent; by 1947 this was reduced to 15 per cent.[14] Nevertheless, the inverse relationship remains clear, although

[14] The corresponding figures for "all marital classes" are 29 per cent in 1940 and .19 in 1947. These per cent differences are simply a crude empirical method of deriving estimates (in single number form) of the "spread" or variation among a group of rates for the purpose of comparing series of the same rates for different time periods. The conventional method of computing individual rates as ratios of a given rate, e.g. that of college graduates, or the professional class, has also been employed in the analysis of these data but is not presented here because of space limitations. The inferences drawn from each method are essentially similar.

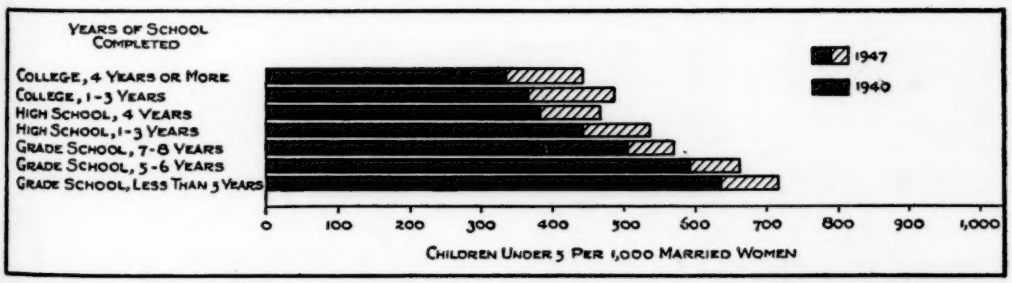

FIGURE 3. Number of Children under 5 per 1,000 Women 15–49 Years Old, Married and Husband Present, by Years of School Completed by the Wife, for the United States, 1947 and 1940. (Ratios standardized for age; see Table 4 for complete description.)

for both periods there is little variation in marital fertility from the high school graduates to the college graduates.

The relation of fertility to occupational class of the husband is presented in detail in Tables 5–7 and charted in Figures 4 and 5. Considering first the average number of children ever born to women of completed fertility (Table 5, Figure 4) it is apparent for each of the three time periods considered (1952, 1940, and 1910) that occupational class [15] and size of family are inversely related. It can be seen from these data that the outstanding exception to this otherwise regular inverse association is the uniformly low fertility of the "clerical and sales" group. It is recognized that this is an "exception" only in the statistical context of a crude ordering of occupational categories on a scale. A comparative cultural analysis of these occupational classes would consider such variables as income, mobility perceptions and aspirations, consumption expenditures and requirements, the role of women, and other factors which would give sociological meaning to these observed occupational fertility differences.

Although the *pattern* of differential fertility by occupation has remained the same for completed fertility up to at least 1952, there has occurred a slight widening of the differences among the occupational classes. By 1910, the average percentage difference between the fertility of each occupational class and the unweighted average of the six classes from "Professional" to "Unskilled" [16] was 14 per cent; by 1940, 17 per cent; and, by 1952,

[15] In order to assure some comparability with other analyses of differential fertility by occupation, the six occupational classes from "Professional" to "Unskilled" are referred to primarily (i.e. exclusive of "Service" and farm occupations).

The years 1952, 1940, and 1910, represent, of course, only the terminal points of this fertility experience. The environmental conditions affecting this fertility are earlier in time. Thus, the women whose completed fertility is shown for 1910 actually could have had these children anywhere from 1850 to 1910.

[16] The average percentage differences for all nine classes was 15 per cent in 1910, 21 per cent in 1940, and 21 per cent in 1952.

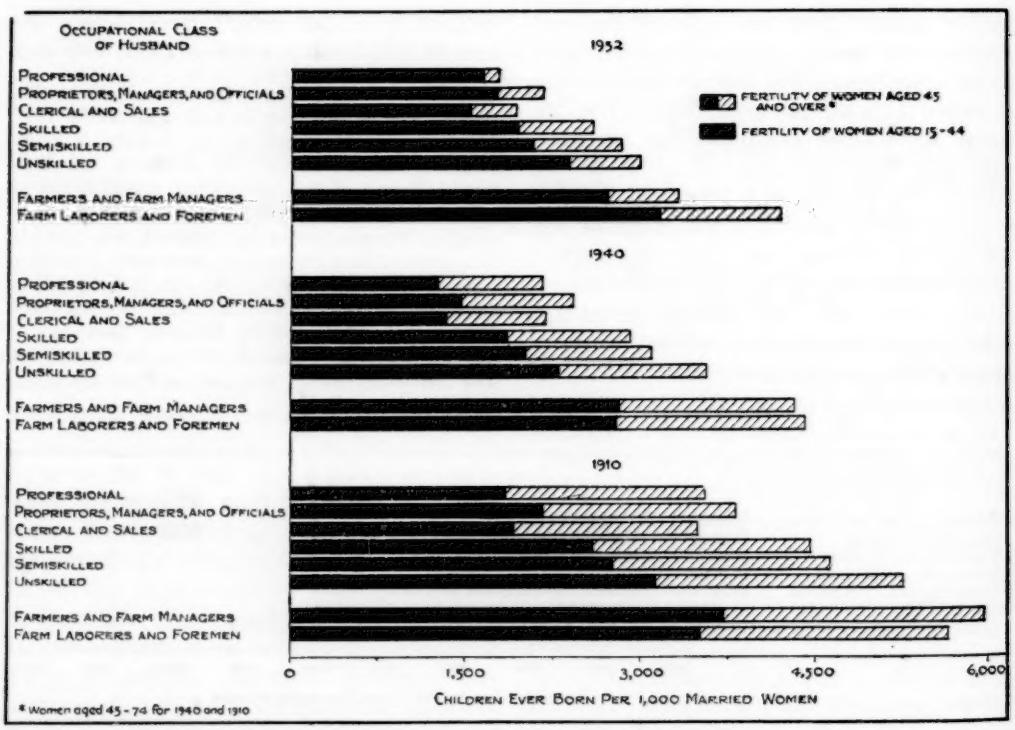

FIGURE 4. Number of Children Ever Born per 1,000 Married Women 15–44 Years Old and 45 Years of Age and Over by Occupational Class of the Husband for the United States, 1952, 1940, and 1910. (Rates standardized for age; see Tables 5–6 for complete description.)

TABLE 5. NUMBER OF CHILDREN EVER BORN PER 1,000 WOMEN OF COMPLETED FERTILITY,[1] MARRIED WITH HUSBAND PRESENT,[2] FOR THE UNITED STATES, 1952, 1940, AND 1910, AND FOR NATIVE-WHITES AND NEGROES, 1940 AND 1910, BY OCCUPATIONAL CLASS OF HUSBAND

Occupational Class [3] of Husband	United States						Native-Whites			Negroes		
				Per Cent of Change					Per Cent of Change			Per Cent of Change
	1952 [5]	1940 [6]	1910 [6]	1940–1952	1910–1940	1910–1952	1940	1910		1940	1910	
Total employed [4]	2,568	3,165	5,024	—19	—37	—49	3,100	4,817	—36	4,287	7,416	—42
Professional	1,780	2,155	3,559	—17	—39	—50	2,139	3,479	—39	2,732	5,741	—52
Proprietors, managers, officials	2,104	2,410	3,803	—13	—37	—45	2,410	3,789	—36	2,349	5,289	—56
Clerical and sales	1,932	2,175	3,524	—11	—38	—45	2,172	3,506	—38	2,453	5,661	—57
Skilled	2,575	2,878	4,458	—10	—35	—42	2,868	4,408	—35	3,433	6,251	—45
Semi-skilled	2,828	3,087	4,643	— 8	—34	—39	3,074	4,575	—33	3,392	6,089	—44
Unskilled	2,991	3,561	5,251	—16	—32	—43	3,592	5,076	—29	3,400	6,155	—45
Service	2,636	2,824	4,391	— 7	—36	—40	2,872	4,381	—34	2,479	4,454	—44
Farmers and farm managers	3,302	4,330	5,965	—24	—27	—45	4,221	5,678	—26	5,445	8,391	—35
Farm laborers and foremen	4,194	4,394	5,651	— 5	—22	—26	4,190	5,231	—20	5,055	7,048	—28

[1] Rates for 1952 based on women 45 years old and over; rates for 1940 and 1910 based on women 45–74 years old.

[2] Rates for 1940 and 1910 based on data for women married once.

[3] Data from the 1910 Census classified according to the 1940 Census occupational classification. Data from the 1952 survey classified according to the 1950 Census occupational classifications which differ in some minor details but are essentially similar to those of the 1940 Census.

[4] Totals for 1910 refer to "husband gainfully occupied."

[5] Rates for 1952 include estimates of children ever born for women not reporting, which tends to lower these rates slightly in comparison with the rates for 1940 and 1910 which are based exclusively on women reporting on number of children.

[6] Rates for 1940 and 1910 United States totals based on native-white and Negro population (comprising 91 per cent of the total female population in 1940 and close to 87 per cent in 1910).

Sources: Rates for 1952 adapted from U. S. Bureau of the Census, *Current Population Reports—Population Characteristics*, Series P-20, No. 46 (December 31, 1953) p. 13. Rates for 1940 and 1910 computed from data in U. S. Bureau of the Census, *Sixteenth Census of the United States: 1940, Population, Differential Fertility 1940 and 1910, Fertility by Duration of Marriage*, Washington, 1947, pp. 33–57.

TABLE 6. NUMBER OF CHILDREN EVER BORN PER 1,000 WOMEN 15–44 YEARS OF AGE, MARRIED WITH HUSBAND PRESENT,[1] FOR THE UNITED STATES, 1952, 1940, AND 1910, AND FOR NATIVE-WHITES AND NEGROES, 1940 AND 1910, STANDARDIZED FOR AGE,[2] BY OCCUPATIONAL CLASS OF HUSBAND

Occupational Class [3]	United States						Native-Whites			Negroes		
				Per Cent of Change					Per Cent			Per Cent
				1940–	1910–	1910–			of			of
of Husband	1952 [6]	1940 [7]	1910 [7]	1952	1940	1952	1940	1910	Change	1940	1910	Change
Total employed [4]	1,985	1,927	2,915	+ 3	−34	−32	1,879	2,808	−33	2,521	3,868	−35
Professional	1,653	1,266	1,818	+31	−30	− 9	1,262	1,783	−29	1,415	2,634	−46
Proprietors, managers, officials	1,759	1,459	2,164	+21	−33	−19	1,457	2,159	−33	1,678	2,482	−32
Clerical and sales [5]	1,555	1,325	1,887	+17	−30	−18	1,323	1,881	−30	1,429	2,379	−40
Skilled	1,932	1,842	2,575	+ 5	−28	−25	1,837	2,561	−28	2,008	2,979	−33
Semi-skilled	2,076	2,001	2,765	+ 4	−28	−25	2,001	2,747	−27	2,019	3,054	−34
Unskilled	2,380	2,283	3,131	+ 4	−27	−24	2,323	3,139	−26	2,157	3,121	−31
Service	1,805	1,645	2,256	+10	−27	−20	1,666	2,321	−28	1,554	2,069	−25
Farmers and farm managers	2,704	2,826	3,727	− 4	−24	−27	2,687	3,559	−25	3,728	4,797	−22
Farm laborers and foremen	3,153	2,803	3,522	+12	−20	−10	2,754	3,371	−18	2,945	3,965	−26

[1] Rates for 1940 and 1910 based on data for women married once.

[2] All rates standardized by the age distribution of married women 15–44 years old with husband present, 1952.

[3] Data from the 1910 Census classified according to the 1940 Census occupational classifications. Data from the 1952 survey classified according to the 1950 Census occupational classifications which differ in some minor details but are essentially similar to those of the 1940 Census.

[4] Totals for 1910 refer to "husband gainfully occupied."

[5] Rate for 1952 estimated from the separate standardized rates for "clerical and kindred workers" and "sales workers."

[6] Rates for 1952 include estimates of children ever born for women not reporting, which tends to lower these rates slightly in comparison with the rates for 1940 and 1910 which are based exclusively on women reporting on number of children.

[7] Rates for 1940 and 1910 United States totals based on native-white and Negro population (comprising over 91 per cent of the total female population in 1940 and close to 87 per cent in 1910).

Sources: Rates for 1952 adapted from U. S. Bureau of the Census, *Current Population Reports—Population Characteristics*, Series P-20, No. 46 (December 31, 1953) p. 13. Rates for 1940 and 1910 computed from data in U. S. Bureau of the Census, *Sixteenth Census of the United States: 1940, Population, Differential Fertility 1940 and 1910, Fertility by Duration of Marriage,* Washington, 1947, pp. 33–57.

18 per cent. This pattern of fertility change is consistent with the hypothesis that contraceptive practices were first employed extensively by the white-collar classes and only gradually were adopted by the working classes. The inverse relation of fertility to occupation and the widening of the differences in completed fertility among occupational classes from 1910 to 1952 can be understood in terms of the more rapid rates of decrease in the fertility of the white-collar classes (see columns on per cent of change in Table 5).

groups averaged only about a 4 per cent increase. The net effect of this reversal in rates of change (which results statistically in greater decreases in the blue-collar occupations over the entire span from 1910 to 1952) has been to produce a narrowing of the differential. Thus, although the average percentage difference between the occupational fertility rates and the average was 18 per cent in 1910 and 20 per cent in 1940, it had dropped to only 12 per cent by 1952. This change is less pronounced if we consider all nine occupational classes instead of only

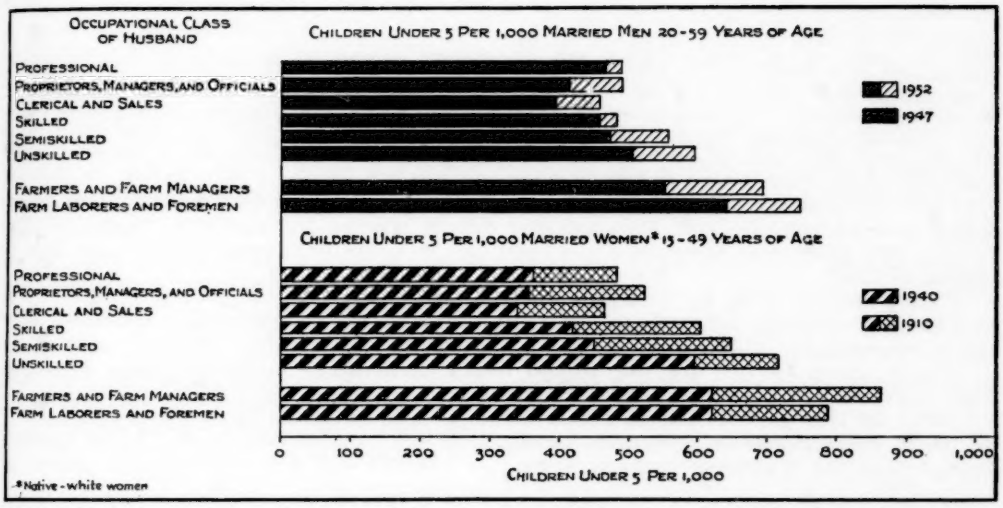

FIGURE 5. Number of Children under 5 per 1,000 Married Men 20–59 Years Old for the United States, 1952 and 1947; and Number of Children under 5 per 1,000 Native-White, Married Women 15–49 Years Old, 1940 and 1910, by Occupational Class of the Husband. (Ratios standardized for age; see Table 7 for complete description.)

Turning to an analysis of the fertility of women in their reproductive years (15–44 years old), the same pattern of differential rates of decline can be discerned between 1910 and 1940 (see Table 6), as was observed for women of completed fertility. However, these women who were in this age group in 1940 and 1952 (a considerable proportion of them at least) participated in the baby boom. Moreover, women with husbands in the white-collar occupations experienced proportionately greater fertility increases than women whose husbands were in "blue-collar" occupations. Collectively, the former averaged over a 20 per cent increase in children ever born between 1940 and 1952, while the skilled, semi-skilled, and unskilled

the six classes from the professional to the unskilled group, due to the increase in fertility among "farm laborers and foremen" and the continued decrease among "farmers and farm managers." Differential fertility by occupation nevertheless has contracted, but the contraction of these rates is due wholly to differential rates of change within the more exclusively urban occupations.

The fertility of women 15 to 44 in 1952 reflects more recent trends than the fertility of women over 45 years of age. Even more sensitive to developments in later years are the ratios of children under 5 to parents of reproductive ages. These ratios for married men 20–59 in 1952 and 1947 and married women 15–49 in 1940 and 1910 are presented

TABLE 7. NUMBER OF CHILDREN UNDER 5 PER 1,000 MEN 20–59 YEARS OLD, MARRIED WITH WIFE PRESENT FOR 1952 AND 1947, AND PER 1,000 NATIVE-WHITE WOMEN 15–49 YEARS OLD, MARRIED ONCE WITH HUSBAND PRESENT FOR 1940 AND 1910, STANDARDIZED FOR AGE,[1] BY OCCUPATIONAL CLASS OF HUSBAND

Occupational Class [2] of Husband	Children Under 5 per 1,000 Men 20–59		Per Cent of Change	Children Under 5 per 1,000 Women 15–49		Per Cent of Change
	1952	1947	1947–1952	1940	1910	1910–1940
Total Employed [3]	529	467	+13	458	679	−33
Professional	492	470	+ 5	366	481	−24
Proprietors, managers, officials	492	417	+18	356	525	−32
Clerical and sales [4]	457	396	+15	346	467	−26
Skilled	480	459	+ 5	422	606	−30
Semi-skilled	555	474	+17	451	649	−31
Unskilled	597	507	+18	598	715	−16
Service	516	410	+26	379	517	−27
Farmers and farm managers	687	555	+24	622	866	−28
Farm laborers and foremen	747	639	+17	621	782	−21

[1] Ratios for 1952 and 1947 standardized by the age distribution of married men aged 20–59 with wife present in 1952. The standard age distribution for 1940 and 1910 ratios is that of married women aged 15–49 with husband present in 1952.

[2] There are some minor classification differences. The 1952 data are based on the 1950 Census classification of occupations; the 1947, 1940, and 1910 data are based on the 1940 Census classification. The 1952 and 1947 ratios apply to employed men only; the 1940 data to all men, and the 1910 data to "gainful workers."

[3] Totals for 1910 refer to "husband gainfully occupied."

[4] Estimated for 1952 and 1947 from ratios presented separately for "clerical and kindred workers" and "sales workers."

Sources: Ratios for 1952 and 1947 adapted from U. S. Bureau of the Census, *Current Population Reports—Population Characteristics,* Series P-20, No. 46 (December 31, 1953) p. 22.

Ratios for 1940 and 1910 computed from data in U. S. Bureau of the Census, *Sixteenth Census of the United States, Population, Differential Fertility, 1940 and 1910, Women by Number of Children Under 5 Years Old,* Washington, 1945, pp. 81–84.

in Table 7.[17] These data on current fertility are unfortunately not comparable for the total time period covered, but they are directly comparable between 1947 and 1952, and between 1910 and 1940. The rates of change within each of these two time periods do not reveal any regular pattern. Actually, the comparison of 1947 with 1952 is not too meaningful in this context in that it is a comparison within the baby-boom decade without reference to the 1940 and earlier fertility rates. Although the series for 1952 and 1947 are not directly comparable [18] to those for 1940 and 1910, one can roughly infer from comparisons between them that the increase in fertility during the baby boom was greatest among the white-collar classes. Even without this crude comparison, examination of the variations among occupational fertility ratios (from professional to unskilled) for each period indicates an average difference in ratios of 14 per cent in 1910, 16 per cent in 1940, and between 7 and 8 per cent for 1947 and 1952.[19] This analysis of current

[17] The use of these denominators for the ratios and the different age groups covered as compared to the previous analysis of color and urban-rural differentials (women 20–44) was necessitated by the Census Bureau's use of these statistics in the recent publications.

[18] The ratios for 1940 and 1910 are based on native-white women only and on women 15–49 years old, while those for 1952 and 1947 refer to the total population and are based on men 20–59 years old.

[19] The corresponding averages for the total nine occupational classes are 19 per cent in 1910, 22 per cent in 1940, and approximately 13 per cent for 1947 and 1952. The fertility of "farmers and farm managers" increased at a slightly higher rate (24 per cent) between 1942–1947 and 1947–1952 than the fertility of "farm laborers and foremen" (17 per cent).

fertility supports the conclusions, from the analysis of children ever born to women 15 to 44; that differential fertility among occupational classes is in the process of contraction.

If this pattern of change continues into the future, that is, if the higher occupational classes increase their fertility proportionately more than the lower classes,[20] or if a pattern of fertility decline once again returns in which the manual or blue-collar group fertility declines at a more rapid rate than that of the white-collar class, we can look foward either to an eventual leveling of fertility rates or even a change from an inverse to a direct relationship. Evidence of this change in pattern can be seen in Figure 5. In 1952, for example, the fertility ratios for the top two occupational classes were identical, and even exceeded that of the skilled as well as the "clerical and sales" occupational group. This change is consistent with the findings in the Indianapolis Study of a direct relationship between socio-economic status and fertility among couples who *planned* size of family.[21] The empirical observation of a direct relationship, however, does not explain it. Some advances on the more complex theoretical front have been made in the Indianapolis Study; others will be made, it is hoped, in the near future.

SUMMARY

The general consensus of demographers and other observers of population phenomena has been that differential fertility among various groupings within the American population has been in a process of contraction during recent decades. The evidence presented in this study, in general, supports this consensus although not without reservations. More precisely, the main findings of this study can be specified as follows. There appears to be some evidence of contraction between white and nonwhite fertility, although since 1920 the relationship has stabilized at around 85 per cent of unity. The evidence on the urban-rural differential is more inconclusive. Although the urban-rural fertility ratios in 1950 are the closest they have ever been in this time series (around 70 per cent of unity), this is due to the greater increases in urban fertility during the baby boom and is not prefaced by any observable trend toward contraction before 1940 except among nonwhites. The data on the educational fertility differential are inadequate. The available series show definite signs of contraction between 1935–1940 and 1942–1947. The main generalization that can be offered about the occupational fertility differential is that following an earlier period in which the differential widened due to a more rapid decrease in the rates of the white-collar classes, there has occurred a definite contraction largely because of the greater rates of increase of this group in the baby-boom period. One result of this differential pattern of change has been to accentuate the alteration of the traditional inverse relation of fertility to occupation. Should this trend continue in the future, students of differential fertility some day may well be seeking explanations of a direct rather than an inverse relationship between education, occupation and fertility.

[20] The rates of change between 1947 and 1952 do not support this presumption.

[21] *Cf.* Kiser and Whelpton: Social and Psychological Factors Affecting Fertility IX. "Fertility Planning and Fertility Rates by Socio-Economic Status."

THE UNWED MOTHER AND SAMPLING BIAS *

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A PRELIMINARY survey of the available data on unwed motherhood reveals that historically a variety of etiological factors have been emphasized. Studies made during the 1920's stressed such causal factors as "immorality" and "mental deficiency." [1] During the 1930's the findings pointed to the factors of a "broken home," "poverty," "little education," and "domestic occupation." [2] Within the past two decades, unwed motherhood has been explained increasingly as an accepted pattern of life in a given sub-culture.[3] However, the most frequent emphasis at the present time appears to be upon psychological processes. The majority of current studies conclude that unwed motherhood is a product of unresolved parent-child conflict and represents an "unrealistic way out of inner difficulties." [4]

Equally evident from such a survey is the fact that in the majority of investigations reported, the samples of unwed mothers were taken from public institutions, welfare agencies or psychiatric clinics. Out of 48 studies surveyed, 39 or 81.3 per cent used samples of unwed mothers taken from psychiatric clinics, social agencies, charity institutions and private therapy cases. Another 10.4 per cent of the studies drew samples from a particular socio-economic or ethnic sub-group, and the remaining 8.3 per cent comprised statistical analyses of available data from a national or international level of incidence.

This method of sampling has prolonged the picture of the unwed mother as being an extremely young, poor, uneducated or psychologically disturbed female. This portrayal has persisted despite the impressions of many professional people working with the unwed mother that this is a phenomenon occurring quite frequently among middle-income, normal and well-educated women.

In an effort to examine unwed motherhood on the basis of a non-agency, non-psychiatric and non-institutional sample, the following study was undertaken.

* Paper read at the annual meeting of the American Sociological Society, September, 1954. A preliminary investigation out of which has developed a research project involving the collection of data from over 500 cases of unwed motherhood as they occur during 1954.

[1] See W. E. McClure, "Intelligence of Unmarried Mothers," *Psychological Clinic,* XX (1931), pp. 154–157; W. E. McClure and B. Goldberg, "Intelligence of Unmarried Mothers," *Psychological Clinic,* XVIII (1929), pp. 119–127; P. G. Kammerer, *The Unmarried Mother,* Boston: Little, Brown and Company, 1918; Willystine Goodsell, *Problems of the Family,* rev. ed., New York: 1936, p. 365; and G. B. Mangold, *Children Born Out of Wedlock,* Columbia: University of Missouri Studies, III, No. 3, June 1921, p. 41.

[2] See Ruth Nottingham, "A Psychological Study of Forty Unmarried Mothers," *Genetic Psychology Monographs,* XIX (May, 1937), pp. 155–228; Ruth Reed, *The Illegitimate Family in New York City,* New York: Columbia University Press, 1934, pp. 138–139; and D. F. Puttee and M. R. Colby, *The Illegitimate Child in Illinois,* Chicago: University of Chicago Press, 1937, pp. 119–120.

[3] See Franklin Frazier, *The Negro in the United States,* New York: Macmillan Company, 1949, pp. 318–321; Hortense Powdermaker, *After Freedom: A Cultural Study in the Deep South,* New York: Viking Press, 1939, pp. 166–170, 204–206; C. S. Johnson, *Growing Up in the Black Belt,* Washington, D.C.: American Council on Education, 1941, Chapter 8; and H. Hertz and S. W. Little, "Unmarried Negro Mothers in a Southern Urban Community," *Social Forces,* XXIII (October, 1944), pp. 73–79.

[4] See Florence Clothier, "Psychological Implications of Unmarried Parenthood," *American Journal of Orthopsychiatry,* XIII (July, 1943), pp. 531–549; Helene Deutsch, *The Psychology of Women, A Psychoanalytic Interpretation,* Vol. II, New York: Grune and Stratton, 1945, Chapter 10; C. Donnell and S. J. Glick, "Background Factors in 100 Cases of Jewish Unmarried Mothers," *The Jewish Social Service Quarterly,* XXIX (Winter, 1952), pp. 152–160; Leontine Young, *Out of Wedlock,* New York: McGraw-Hill, 1954; B. Hutchinson, "Unmarried Mothers as Patients of a Psychiatric Clinic," *Smith College Studies in Social Work,* XIX (February, 1949), pp. 102–103; Norman Reider, "The Unmarried Father," *American Journal of Orthopsychiatry,* XVIII (1948), pp. 230–237; and J. Kasanin and Sieglinde Handschin, "Psychodynamic Factors in Illegitimacy," *American Journal of Orthopsychiatry,* XI (January, 1941), pp. 66–84.

THE SAMPLE

A questionnaire was sent to all surgeons, obstetricians, gynecologists, general practitioners and osteopaths listed in the 1952 medical directory of Alameda County, California. The questionnaire requested data on the mothers of all babies born out of wedlock which the doctors had delivered during 1952 in *private practice* (i.e., not delivered in a county hospital, clinic or public institution). Of the 576 questionnaires mailed, 409 or 71 per cent were returned. Of the 409 doctors who responded, 31.8 per cent had delivered in private practice during 1952 a total of 252 babies born out of wedlock.

These 252 cases of illegitimate births were further divided into three categories: (a) 171 unwed mothers who had never been married, (b) 51 mothers who were divorced or separated from their husbands, and (c) 30 mothers who were married but the baby was fathered by a man other than the mother's legal husband. The data being reported concern 137 unwed mothers of category "a" for whom data were reported by the doctors.[5] Of these 137 unwed mothers, 83.9 per cent were white, 13.1 per cent Negro, 2.2 per cent Oriental, and for 0.7 per cent no data were given. This represented the first child born out of wedlock for 87.6 per cent of the 137 unwed mothers, the second for 8.0 per cent, the third for 2.9 per cent, and for 1.5 per cent no data were given.

SOCIO-ECONOMIC STATUS

The extreme youth of the unwed mother, which has been pointed out in the majority of previous studies, would appear from Table 1 to be more typical of the institutional case than of the private practice case (hereafter referred to as PR). Rather than cite the findings of other studies using institutional samples, comparisons are limited to the Booth Memorial Hospital data since they occurred during the same year within the same county. Booth Memorial Hospital is the Salvation Army Hospital. Whereas 80.4 per cent of the unwed mothers delivered at Booth were 21 years of age or younger, 51.8 per cent of the PR cases were 22 years of age or older. This difference would be even more striking if the Booth cases were not inclusive of divorced and widowed mothers, since the PR sample includes only unwed mothers who have never been married. The alleged sexual mate reported in PR cases also tends to be older than the alleged sexual mate reported in institutional studies.

Table 2 further illustrates how investigations using unwed mothers from institutions and agencies may reveal factors which are more closely related to a screening process than they are related specifically to unwed motherhood. Of the unwed mothers

TABLE 1. AGE OF THE UNWED MOTHERS AND THEIR ALLEGED SEXUAL MATES RECORDED IN PR DELIVERIES, AS COMPARED WITH THE AGE OF THE UNWED MOTHERS AND THEIR ALLEGED SEXUAL MATES RECORDED IN BOOTH HOSPITAL DELIVERIES

	Unwed Mothers			Alleged Sexual Mates	
Ages [1]	PR Cases N=137	Booth Cases [2] N=270	Ages [1]	PR Cases N=137	Booth Cases N=270
	Per cent	Per cent		Per cent	Per cent
13–17	15.4	46.3	14–16	1.5	1.9
18–21	32.8	34.1	17–20	11.7	29.3
22–25	33.6	11.1	21–25	14.6	32.2
26–30	13.1	5.9	26–30	18.2	16.6
31 and over	5.1	2.6	31 and over	27.0	9.3
No information	....	....	No Information	27.0	10.7
	100.0	100.0		100.0	100.0

[1] Age categories are divided for purposes of comparison with the available data from Booth.
[2] The writer is indebted to Brigadier Cox for the Booth data.

[5] In the other 34 cases of category "a" the doctors had indicated delivering such an unwed mother but did not complete the questionnaire.

delivered in private practice, 38 per cent had attended or completed college and 26.2 per cent had not completed high school. For the unwed mothers delivered at Booth the percentages were 12.2 per cent and 52.2 per cent respectively. A comparison of the educational attainment of the alleged sexual mates in the two groups also indicates a higher educational attainment for the alleged father of the baby delivered in private practice than for the alleged father of the baby delivered at Booth.

The educational attainment of the unwed mothers' parents (Table 3), as well as the occupation of their fathers (Table 4), suggests that the educational and occupational level of many of these unwed mothers is less indicative of recent social climbing and more indicative that they come from "established" middle-class homes.

TABLE 2. EDUCATION OF UNWED MOTHERS AND THEIR ALLEGED SEXUAL MATES RECORDED IN PR DELIVERIES, COMPARED WITH THE EDUCATION OF UNWED MOTHERS AND THEIR ALLEGED SEXUAL MATES RECORDED IN BOOTH HOSPITAL DELIVERIES

Education	Unwed Mothers		Alleged Sexual Mates	
	PR Cases N = 137	Booth Cases N = 270	PR Cases N = 137	Booth Cases N = 270
	Per Cent	Per Cent	Per Cent	Per Cent
Attended or completed college	38.0	12.2	35.8	21.0
Completed high school	29.2	29.3	18.2	27.9
Less than 12th grade	26.2	52.2	8.8	27.4
No information or other	6.6	6.3	37.2	23.7
	100.0	100.0	100.0	100.0

RESIDENCE AS A FACTOR IN BIASED SAMPLING

The policy which many welfare agencies and public institutions observe of not accepting "non-residents" appears to be operative as an additional factor in biasing the samples of unwed mothers taken from such agencies and institutions. For example, in the present study, as indicated in Table 5, the higher the educational attainment of the unwed mother, the more likely that she came from out of the state to have her baby in Alameda County. The lower her educational attainment, the more likely she was to have her baby in the county in

TABLE 3. EDUCATION OF THE PARENTS OF 137 UNWED MOTHERS

Education	Unwed Mother's Father N = 137	Unwed Mother's Mother N = 137
	Per Cent	Per Cent
College graduate	21.1	14.5
Some college	13.2	10.3
High School graduate	13.2	18.2
Less than 12th grade	8.0	5.9
No information	44.5	51.1
	100.0	100.0

TABLE 4. THE OCCUPATIONS OF 137 UNWED MOTHERS, THE OCCUPATIONS OF THEIR FATHERS, AND THE OCCUPATIONS OF THEIR ALLEGED SEXUAL MATES *

Occupational Category	Unwed Mothers N = 137	Fathers of Unwed Mothers N = 137	Alleged Sexual Mate of Unwed Mothers N = 137
	Per Cent	Per Cent	Per Cent
Professional	11.0	14.6	16.8
Semi-professional	3.6	0.7	.....
Managerial	0.7	9.5	8.8
College students	15.3	.....	8.8
Clerical	27.7	.....	.....
Sales	2.2	7.4	5.8
Armed services	.....	.....	5.8
High school and grammar school	11.0	.....	5.1
Skilled	3.0	9.0	7.3
Semi-skilled	5.2	7.4	7.3
Unskilled	3.6	2.1	5.1
Not working	6.5	.....	.....
No information	10.2	49.3	29.2
	100.0	100.0	100.0

* Occupational classification is taken from *Dictionary of Occupational Titles Vol. II Occupational Classifications*, 2nd ed., Washington: U: S. Government Printing Office, 1949.

which she resides. Thus the studies which rely on agency and institutional samples tend to miss the unwed mother who is presumably in an educational and financial position to travel to another state in order to have her baby delivered away from home in the secrecy of a doctor's private practice.

TABLE 5. THE RESIDENCE AND EDUCATIONAL ATTAINMENT OF 137 UNWED MOTHERS

	Permanent Residence		
Educational Attainment	Alameda County N=73	County in California Other than Alameda N=27	State Other than California N=37
	Per Cent	Per Cent	Per Cent
Attended or completed college	19.2	33.3	78.4
Completed high school	32.8	33.3	13.5
Less than 12th grade	39.8	22.2	8.1
No information	8.2	11.2	...
	100.0	100.0	110.0

AGE DIFFERENCES AND EDUCATIONAL ATTAINMENT

There were 94 cases for which age data were given for both the alleged father and the unwed mother. Table 6 shows that the age difference between the unwed mother and the alleged father tends to increase with the increase in the unwed mother's educational attainment. It is frequently assumed that the unwed mother is usually a very young girl who is exploited by a man much older than she, and that the less education she has the more likely she is to be exploited by an older man. However, the following table indicates that 70 per cent of the unwed mothers who had less than a 12th grade education were mated sexually with a man who was at the most two years older than themselves. Only 50 per cent of the unwed mothers who had attended or completed college had as the father of their baby a man who was seven or more years older than themselves. Some psychiatric and psychological analyses have interpreted this age difference to mean that the unwed mother is using the sexual mate to work through earlier unresolved emotional relationships with her own father. On the basis of the above data the question arises as to why this age difference does not apply equally to the less and the better educated unwed mother.

TABLE 6. EDUCATION OF 94 UNWED MOTHERS COMPARED WITH THE AGE DIFFERENCE BETWEEN HER ALLEGED SEXUAL MATE AND HERSELF

Age Differences	Attended or Completed College N=44	Completed High School N=30	Less Than 12th Grade N=20
	Per Cent	Per Cent	Per Cent
Female older	4.5	....	5.0
Same age as man or man is 1–2 years older	16.0	13.3	70.0
Man is 3–6 years older	29.5	66.7	15.0
Man is 7 or more years older	50.0	20.0	10.0
	100.0	100.0	100.0

TABLE 7. EDUCATION OF 86 UNWED MOTHERS COMPARED WITH THE EDUCATION OF THE ALLEGED SEXUAL MATE IN EACH CASE *

	Unwed Mother's Education		
Alleged Father's Education	Attended or Completed College N=40	Completed High School N=30	Less Than 12th Grade N=16
	Per Cent	Per Cent	Per Cent
Attended or completed college	90.0	30.0	25.0
Completed high school	7.5	60.0	25.0
Less than 12th grade	2.5	10.0	50.0
	100.0	100.0	100.0

* Chi-square for this table is 49.02 with p less than .001. The association thus appears highly significant, even though the expected values are less than 5 in some cells.

EDUCATIONAL RELATIONSHIPS

There were 86 cases for which data were also available on the education of both the alleged father and the unwed mother. The data from Tables 6 and 7 suggest that the less educated unwed mother is more

often mated with a sexual mate who is the same age but who has more education than she has. The better-educated unwed mother is more often mated with a sexual mate who is older than she, but who has the same educational attainment.

OCCUPATIONAL ASSOCIATION

There were 89 cases for which data were available on the occupation of both the unwed mother and the alleged sexual mate. This is in rather sharp contrast to the findings from investigations of unwed mothers taken from clinics and institutions, which emphasize that the unwed mother knows very little or at least gives very little information about the alleged sexual mate. This may again suggest that the apparent disinterest in the alleged sexual mate is true of only certain samples of unwed mothers. In view of Durkheim's prediction that the occupational group would become the "primary" group in the industrialized and urbanized society, it is interesting that occupational association appears to be operative as a situational factor in some of these cases of unwed motherhood.

TABLE 8. OCCUPATIONS OF 89 UNWED MOTHERS AND THEIR ALLEGED SEXUAL MATES

Number of Cases	Occupation of Unwed Mother	Occupation of Alleged Sexual Mates
11	college student	college student
6	high school student	high school student
6	stenographer and secretary	executive or office manager
4	teacher	teacher
3	nurse	physician
3	sales clerk	salesman
2	legal stenographer	lawyer
2	medical receptionist	dentist
2	beauty operator	hair stylist and barber
2	cashier	cafe owner and butcher
2	waitress	cafe owner and bartender
2	bus driver	bus driver
2	PBX and telephone operator	lineman and installer
2	factory worker	factory worker
1	waitress	waiter
1	reporter	reporter
1	office clerk	store manager
1	domestic	her employer
1	usher	theater manager
1	lab technician	chemist
1	actress	musician
1	servicewoman	serviceman
1	buyer (clothing)	merchandiser
1	stenographer	accountant
1	secretary	dentist
1	designer (clothes)	store manager
1	secretary	chemist
27	no apparent relationship	
89		

SUMMARY

A preliminary survey of the investigations of unwed motherhood indicated that the majority of the samples studied have been taken from public institutions, welfare agencies and psychiatric clinics. This method of sampling has prolonged the picture of the unwed mother as being an extremely young, poor, uneducated or psychologically disturbed female. This sampling procedure is in part related to the ease with which such groups can be studied. It is also related to, and in turn reinforces, a like-causes-like approach which tends to regard unwed motherhood as bad and then emphasizes bad or pathological etiological factors.[6] Historically, the factors emphasized have ranged from mental deficiency in the 1920's to psychological disturbances at the present time.

The findings reported in the present paper resulted from a deliberate attempt to sample unwed mothers who do not go to agencies, clinics or institutions. The data were based on a 71 per cent response from 576 doctors who provided information on 137 unwed mothers delivered in private practice in Alameda County, California, during 1952.

The following findings suggest the need for more inclusive samples in studies of unwed motherhood and raise additional questions for future research. Of the 137 unwed mothers delivered in private practice:

83.9 per cent were white;
51.8 per cent were 22 years of age or older;
38.0 per cent had attended or completed college and 34.3, 24.8, and 35.8 per cent respectively of their fathers, mothers and alleged sexual mates had attended or completed college;

[6] See the excellent discussion by Kingsley Davis of the "evil causes evil" fallacy in "Illegitimacy and the Social Structure," *American Journal of Sociology*, XLV (September, 1939), pp. 215–233.

60.5 per cent were employed in professional or white collar jobs or were college students, and only 8.8 per cent were employed in semi-skilled or unskilled jobs;

36.5 per cent of 74 who were working, received a salary of 251.00 dollars or more per month;

78.4 per cent of those who came from out of the state to have their baby in California had attended or completed college;

50.0 per cent of those who had attended or completed college were mated sexually with a man seven or more years their senior;

70.0 per cent of those with less than a 12th grade education were mated sexually with a man the same age or not more than two years their senior;

90.0 per cent of those who had attended or completed college were mated with an alleged sexual mate who had attended or completed college.

Occupational association appeared to be operative as a situational factor in some of the 89 cases for which occupational data were available for both the unwed mother and the alleged sexual mate.

THE SOCIAL ISOLATION HYPOTHESIS AND SCHIZOPHRENIA *

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The pathogenic effects of social isolation have long been recognized by specialists in social and personal disorganization. Faris has connected social isolation specifically with the onset of schizophrenia, the most common type of mental disorder.[1] The schizophrenic is generally depicted as one who lives in a mental world of his own fancy, a sort of "pseudo-community" [2] inhabited by creatures whom the schizophrenic considers desirable or perhaps at least subject to his control. Since he thereby loses contact and communication with others, the nexus between social isolation and this type of psychotic is plausible. However, no empirical test of the relationship between this social condition and schizophrenia has been made.[3] Furthermore, even if isolation is a valid etiological factor in this disorder, the elements or characteristics of such lack of social contact in their specific connection with schizophrenia are little known.

A useful distinction between "precipitating" and "predisposing" factors in the etiology of mental illness may be recognized.[4] The former includes those factors which accelerate the onset of the disorder while the latter are composed of those factors which present a susceptibility, tendency, or predilection toward acquiring the breakdown.[5] It seems plausible to assign

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[1] See R. E. L. Faris, "Cultural Isolation and the Schizophrenic Personality," *American Journal of Sociology,* XXXIX (September, 1934), pp. 155–169; R. E. L. Faris and H. W. Dunham, *Mental Disorders in Urban Areas,* Chicago: University of Chicago Press, 1939, Ch. 11; and R. E. L. Faris, *Social Disorganization,* New York: Ronald Press, 1948, Ch. 9.

[2] N. A. Cameron and A. Magaret, *Behavior Pathology,* Boston: Houghton-Mifflin, 1951, Ch. 13.

[3] An inquiry into the relation between certain aspects of social isolation and the neurotic symptoms of anxiety, hysteria and depression among British college students was recently made (P. Halmos, *Solitude and Privacy,* London: Routledge and Kegan Paul, 1952). Our study, however, is concerned with a more severe disorder.

[4] Such a distinction is used, among others, by E. W. Burgess, "Social Factors in the Etiology and Prevention of Mental Disorders," *Social Problems,* 1 (October, 1953), p. 54; J. D. Page, *Abnormal Psychology,* New York: McGraw-Hill, 1947, p. 86f.; M. A. Elliott and F. E. Merrill, *Social Disorganization,* New York: Harper and Brothers, 1950 (3rd ed.), pp. 281–284; and J. C. Coleman, *Abnormal Psychology and Modern Life,* New York: Scott-Foresman and Co., 1950, p. 136.

[5] A psychiatrist recently defined these factors as follows: *Predisposing factors* refer to "the con-

social factors to the category of precipitating variables in mental disorder,[6] thereby providing a possible reason why some persons acquire mental aberrations while others do not. That is, a person who may possess a tendency to acquire a particular mental illness, but never comes in contact with the agent which may precipitate its onset, will consequently never manifest that aberration. On the other hand, if an individual has no predisposition to acquire a certain disease, then his experience of an event that is precipitant of that disorder will not incur its onset in that person.[7]

A methodological implication from the foregoing is that mental patients do not serve as appropriate subjects of analysis of precipitating influences in mental disorder. Persons who have been committed to mental hospitals and diagnosed as having a mental illness are regarded as representing a mixture of both predisposing and precipitating forces; consequently, they may be considered as being perhaps too highly selective for the determination of precipitating influences. Mental patients, on the other hand, are regarded as more appropriate sources of data primarily for the ascertainment of *predisposing* factors, for the quest, that is, of specific variables that led these particular individuals to acquire their disorders. Since *precipitating* forces are considered as external to the individual, then in investigating such factors one must go beyond such persons and look into their social environment.

This type of inquiry, therefore, does not search for those factors which bring about the onset of a particular psychosis in a particular individual. Rather, this study seeks to determine at least some of those aspects of social isolation in the general population which might precipitate or accelerate the incidence of schizophrenia in any or all persons experiencing or about to experience such conditions. The logic thus implied is that when certain conditions that have been found to be precipitant of schizophrenia exist in a community, the probability of acquiring this disorder is greater for its inhabitants than for the occupants of a community not found to possess these social conditions.

This paper presents the results of a test of some aspects of social isolation as precipitating factors in the schizophrenic psychoses.

THE HYPOTHESES

Defining social isolation as the cutting off or minimizing of contact and communication with others, the major hypothesis states that those communities having high rates of schizophrenia will have a concomitantly high degree of social isolation. The question still remains as to what elements in social interaction may be singled out as conducive to social isolation. With no ready measurement available, the following array of factors, amenable to enumeration, and with no claim to finality or completeness, are regarded as provisional criteria of social isolation. Accompanying null propositions for each of these factors are as follows:

A. *Anonymity.*

Between the inhabitants of high and low-rate schizophrenic areas, there is no difference in:

(1) the knowledge of the names of neighbors;

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ditioning factors in the early years of life . . . that comprehensively shape what has been called the individual's 'basic personality' . . ." while *precipitating factors* refer to "those situational conditions which . . . can operate as sources of strain so severe as to precipitate psychopathological disability," (T. A. C. Rennie, "The Yorkville Community Mental Health Research Study," in *Interrelations Between the Social Environment and Psychiatric Disorders,* New York: Milbank Memorial Fund, 1953, p. 213f.). If certain variables, therefore, in the social structure are considered as accelerating the onset of mental breakdown apart from the susceptibilities of individuals to acquire these disorders, then much cloudy thinking and misplaced criticism against sociologists working in this area can be averted.

[6] See Burgess, *op. cit.*, p. 54.

[7] An extremely sociologistic view might deny the existence of predisposing factors in the etiology of mental disorders. But to deny their existence would mean that should social isolation, for example, be found to precipitate schizophrenia, then *every* person in socially isolated communities logically should become schizophrenic. Incidence rates, that is, of disorders in such areas would necessarily be 100 per cent among the inhabitants of these areas. Since this is obviously untrue, the significance of individual susceptibilities to acquire these disorders must be recognized in addition to the external precipitating factors.

(2) the estimated number of personal friends;

(3) the estimated number of acquaintances.

B. *High Spatial Mobility.*

Between the residents of high and low-rate schizophrenic communities, there is no difference in:

(4) The number of persons renting or owning their places of residence;

(5) the number of times moved within the city;

(6) the number of places lived before migrating into the city;

(7) the length of residence in present abodes.

C. *Remote Location of Friends.*

(8) There is no difference in the location of most of one's friends between occupants of high and low-rate schizophrenic areas.

D. *Low Frequency of Participation in Groups and Institutions.*

Between the inhabitants of high and low-rate schizophrenic communities, there is no difference in:

(9) membership in professional organizations or labor unions;

(10) membership in lodges or fraternal organizations;

(11) the amount of voting in national and city elections of the previous year;

(12) active church membership.

E. *Low Occupational Participation.*

Between the occupants of high and low-rate schizophrenic areas, there is no difference in:

(13) the number of unemployed persons in the working force;

(14) the estimated number of jobs husbands have held;

(15) the number of times having quit or been discharged from a job.

F. *Low Frequency of Interaction with other Communities.*

Between the inhabitants of high and low-rate schizophrenic communities, there is no difference in:

(16) the number of times going downtown during the previous month;

(17) the number of visits with friends in the previous four weeks;

(18) the number of visits to other areas of the city in the preceding four weeks;

(19) the number of trips out of town during the previous month.

The degree of rejection of the above six factors or clusters of elements and their related nineteen null propositions will thus offer an empirical basis for evaluating the degree of tenability of the major social isolation hypothesis.

RESEARCH DESIGN AND PROCEDURE

The foregoing hypotheses are stated in terms of communities exhibiting highest and lowest known rates of schizophrenia. The underlying logic of the design is that if social factors are valid precipitating influences in the incidence of mental disorders, then communities having extreme differences in incidence rates of certain psychoses should have concomitant differentiations in certain social variables. If social isolation, therefore, is a precipitating factor in the etiology of schizophrenia, then communities having high rates of this psychosis should have a correspondingly higher amount of social isolation than low-rate areas. Furthermore, in order for a factor of social isolation to be specifically precipitant of schizophrenia, this same variable cannot discriminate between areas of other psychoses. Therefore, every null proposition rejected for the schizophrenic communities will be tested for areas exhibiting highest and lowest rates of the manic-depressive psychoses, another major functional disorder which is generally held to have different syndromes and etiology.[8] Finally, if a similar significant difference is found for the manic-depressive communities, then an additional test will be made for a difference between the high-rate schizophrenic and manic-depressive areas. If this reveals a significant difference in favor of the hypothesis in question toward the high-rate schizophrenic areas, then consideration of such a factor as particularly connected to schizophrenia is deemed warranted. On the other hand, if a significant difference is found for both schizophrenic and manic-depressive communities, but no difference found between these high-rate areas, then the conclusion that this factor cannot be feasibly regarded

[8] The antithetical aspect of these two disorders has been stressed by Faris and Dunham when they stated that manic-depression is connected "with extremely intimate and intense social contacts. This apparently is just the opposite from the situation of the schizophrenic, where isolation from such contacts appears to be an associated condition" (*op. cit.*, p. 173).

as precipitant specifically of schizophrenia seems justified.

For the period 1940 through 1952, it was found that 668 patients were committed to a local public mental hospital from the city of Austin, Texas.[9] For this period age-corrected incidence rates of schizophrenia and manic-depressive psychosis—the ratio of the number of patients diagnosed in these psychotic categories to the number of inhabitants within the age-ranges of these patients—were computed for each of the city's fifteen census tracts. The two census tracts having the highest rates of schizophrenia and of the manic-depressive psychoses were then separated from the two tracts exhibiting the lowest rates of these disorders.

A systematic sample of these high and low-rate psychotic communities was drawn and their residents so selected were interviewed. Chi-square tests of significance for the six factors, and their related items considered conducive to social isolation between the areas, were computed. The customary five per cent level of significance was the minimal confidence limit in the statistical analysis.

FINDINGS

Thirty-three per cent of the 115 schizophrenic patients were residing in tracts 9 and 10 at the time of their commitment, while 29.5 per cent of the 78 manic-depressive cases were found in tracts 8 and 12. (These four tracts lie adjacent to Austin's central business district and, in the case of the first three of these tracts, extend to the periphery of the city.) Tracts 1 and 15 were the two lowest schizophrenic areas, totaling 3.4 per cent of these patients, while the lowest manic-depressive areas were tracts 5 and 2 with only 2.6 per cent of these cases. (Tracts 1, 15, and 2 extend to the edge of the city, while tract 5 is located midway between the central business district and the northern periphery of the town.) Of special note is the fact that there is no overlapping of these high and low-rate communities for the two psychoses.

The following factors of social isolation were found to be significantly different between the high and low-rate schizophrenic areas:

Anonymity. Residents of the highest-rate schizophrenic communities were found to know significantly fewer names of their neighbors than those in the lowest-rate schizophrenic areas.[10] A significant difference in knowledge of neighbors' names was also found between the occupants of the differential manic-depressive communities. However, a further test revealed that inhabitants of the high-rate schizophrenic areas knew significantly fewer names of their neighbors than those in the high-rate manic-depressive communities. Residents of the high-rate schizophrenic areas also had significantly lower estimates of the number of their personal friends than did the inhabitants of the low-rate schizophrenic areas. No difference in estimates of the number of friends was found between the occupants of the differential manic-depressive communities. While inhabitants of both the high-rate schizophrenic and manic-depressive areas estimated significantly fewer acquaintances than those in the low-rate neighborhoods, a subsequent test indicated that those in the high-rate schizophrenic communities had significantly fewer acquaintances than residents of the high-rate manic-depressive areas.[11] Thus, the first three null hypotheses are rejected. These findings are summarized in Table 1.

Spatial Mobility. This factor in general was not found to be connected to the high-rate schizophrenic communities as anticipated. While both the high-rate schizophrenic and manic-depressive areas had significantly more persons renting and fewer owning their homes than their low-rate counterparts, the high-rate schizophrenic communities contained significantly more in-

[9] For a preliminary ecological study of this city, see I. Belknap and E. G. Jaco, "The Epidemiology of Mental Disorders in a Political-Type City," in *Interrelations Between the Social Environment and Psychiatric Disorders,* New York: Milbank Memorial Fund, 1953, pp. 235–243.

[10] In calculating the theoretical frequencies of chi-squares for this hypothesis, the number of vacant or non-residential structures within the block were omitted, since these conditions would reduce their probabilities of knowing their neighbors.

[11] Halmos found an inverse relation between the number of friends of his subjects and their anxiety scores (*op. cit.*, p. 92).

habitants who were renting than the high-rate manic-depressive neighborhoods.

Residents of the high-rate schizophrenic communities were found to have moved within the city no more than occupants of the low-rate schizophrenic areas. No significant difference in intra-city migration between inhabitants of the differential manic-depressive areas was also found. Occupants of the high-rate schizophrenic communities were found to have lived in significantly *fewer* places before migrating to the city of Austin than those in the low-rate schizophrenic areas. No significant difference in this factor was found for the high and low-rate manic-depressive areas. Consequently, occupants of the high-rate schizophrenic communities indicate a lower amount of inter-city migration than those residing in the low-rate schizophrenic areas.

Inhabitants of both high-rate schizophrenic and manic-depressive communities had a significantly longer length of residence in the city than those in their low-rate counter-areas. A subsequent test revealed, however, that there is no difference in length of residence of the occupants of these high-rate psychotic communities. Table 2 summarizes these findings.

Summarizing for the factor of spatial mobility, two of the four null propositions were rejected, indicating that more residents of the high-rate schizophrenic communities rented than owned homes and have lived

TABLE 1. INDICES OF ANONYMITY BETWEEN SCHIZOPHRENIC AND MANIC-DEPRESSIVE COMMUNITIES

	Knew Names of Neighbors					
	Yes		No		Totals	
	N	Per Cent	N	Per Cent	N	Per Cent
High schizophrenic areas [a]	323	56.9	245	43.1	568	43
Low schizophrenic areas	648	87.8	90	12.2	738	57
High manic-depressive areas [b]	464	73.2	170	26.8	634	54
Low manic-depressive areas	431	78.8	116	21.2	547	46
High schizophrenic areas [c]	323	56.9	245	43.1	568	47
High manic-depressive areas	464	73.2	170	26.8	634	53
	Estimated Number of Friends					
	Above Median		Below Median		Totals	
	N	Per Cent	N	Per Cent	N	Per Cent
High schizophrenic areas [d]	58	37.7	96	62.3	154	45
Low schizophrenic areas	114	60.0	76	40.0	190	55
High manic-depressive areas [e]	79	45.9	93	54.1	172	54
Low manic-depressive areas	80	54.8	66	45.2	146	46
	Estimated Number of Acquaintances					
	Above Median		Below Median		Totals	
	N	Per Cent	N	Per Cent	N	Per Cent
High schizophrenic areas [f]	43	39.1	110	60.9	153	45
Low schizophrenic areas	128	67.7	61	32.3	189	55
High manic-depressive areas [g]	65	37.6	108	62.4	173	56
Low manic-depressive areas	90	65.7	47	34.3	137	44
High schizophrenic areas [h]	64	41.8	89	58.2	153	47
High manic-depressive areas	99	57.2	74	42.8	173	53

[a] Chi-square=161.17; P less than .001
[b] Chi-square=5.01; P less than .05
[c] Chi-square=35.31; P less than .001
[d] Chi-square=16.98; P less than .001 (Median=25)
[e] Chi-square insignificant (Median=21)
[f] Chi-square=53.10; P less than .001 (Median=171)
[g] Chi-square=24.18; P less than .001 (Median=175)
[h] Chi-square=7.58; P less than .01 (Median=88)

in fewer places before coming to the city than inhabitants of the other psychotic areas. Their spatial mobility, consequently, cannot be said to be any higher than residents in other areas.

Social Participation. This factor was also not generally related to the high-rate schizophrenic communities, since only one of the four null hyphotheses concerning this trait was rejected (Table 3). Both the high-rate schizophrenic and manic-depressive areas were found to have significantly fewer members of professional organizations and labor unions than the low-rate com-

TABLE 2. DIFFERENTIAL ASPECTS OF SPATIAL MOBILITY BETWEEN SCHIZOPHRENIC AND MANIC-DEPRESSIVE COMMUNITIES

	Own Homes		Rent Homes *		Totals	
	N	Per Cent	N	Per Cent	N	Per Cent
High schizophrenic areas [a]	2316	46.0	2718	54.0	5034	44
Low schizophrenic areas	4139	65.0	2232	35.0	6371	56
High manic-depressive areas [b]	2383	49.0	2482	51.0	4865	56
Low manic-depressive areas	2263	59.6	1532	40.4	3795	44
High schizophrenic areas [c]	2316	46.0	2718	54.0	5034	51
High manic-depressive areas	2383	49.0	2482	51.0	4865	49
	Number of Moves in City					
	Above Median		Below Median		Totals	
	N	Per Cent	N	Per Cent	N	Per Cent
High schizophrenic areas [d]	88	54.7	73	45.3	161	45
Low schizophrenic areas	91	46.2	106	53.8	197	55
High manic-depressive areas [e]	90	52.6	81	47.4	171	55
Low manic-depressive areas	66	46.8	75	53.2	141	45
	Number of Places Lived Before Coming to the City					
	Above Median		Below Median		Totals	
	N	Per Cent	N	Per Cent	N	Per Cent
High schizophrenic areas [f]	58	36.0	103	64.0	161	45
Low schizophrenic areas	121	61.4	76	38.6	197	55
High manic-depressive areas [g]	81	46.0	95	54.0	176	55
Low manic-depressive areas	78	54.9	64	45.1	142	45
	Length of Residence in Present Abode					
	Above Median		Below Median		Totals	
	N	Per Cent	N	Per Cent	N	Per Cent
High schizophrenic areas [h]	106	69.3	47	30.7	153	44
Low schizophrenic areas	67	34.7	126	65.3	193	56
High manic-depressive areas [i]	100	57.1	75	42.9	175	55
Low manic-depressive areas	58	41.1	83	58.9	141	45
High schizophrenic areas [j]	85	55.6	68	44.4	153	47
High manic-depressive areas	79	45.1	96	54.9	175	53

* Adapted from U. S. Census of Population: 1950, *Census Tract Statistics*, III, ch. 3.
[a] Chi-square=413.09; P less than .001
[b] Chi-square=97.19; P less than .001
[c] Chi-square=8.79; P less than .01
[d] Chi-square insignificant (Median=2)
[e] Chi-square insignificant (Median=2)
[f] Chi-square=22.85; P less than .001 (Median=2)
[g] Chi-square insignificant (Median=2)
[h] Chi-square=40.80; P less than .001 (Median=14)
[i] Chi-square=8.00; P less than .01 (Median=15)
[j] Chi-square insignificant (Median=21)

TABLE 3. DIFFERENTIAL SOCIAL PARTICIPATION BETWEEN SCHIZOPHRENIC AND MANIC-DEPRESSIVE COMMUNITIES

	Professional or Labor Organizations					
	Members		Non-Members		Totals	
	N	Per Cent	N	Per Cent	N	Per Cent
High schizophrenic areas [a]	19	12.8	130	87.2	149	45
Low schizophrenic areas	77	42.1	106	57.9	183	55
High manic-depressive areas [b]	31	19.0	132	81.0	163	54
Low manic-depressive areas	44	31.9	94	68.1	138	46
High schizophrenic areas [c]	19	12.8	130	87.2	149	48
High manic-depressive areas	31	19.0	132	81.0	163	52

	Lodge or Fraternity					
	Members		Non-Members		Totals	
	N	Per Cent	N	Per Cent	N	Per Cent
High schizophrenic areas [d]	16	10.6	135	89.4	151	45
Low schizophrenic areas	92	49.2	95	50.8	187	55
High manic-depressive areas [e]	50	30.7	113	69.3	163	53
Low manic-depressive areas	50	34.5	95	65.5	145	47

	Voting in Two Elections					
	Voting		Non-Voting		Totals	
	N	Per Cent	N	Per Cent	N	Per Cent
High schizophrenic areas [f]	81	25.4	262	74.6	319	45
Low schizophrenic areas	262	66.7	131	33.3	393	55
High manic-depressive areas [g]	103	29.3	249	70.7	352	55
Low manic-depressive areas	146	50.3	144	49.7	290	45
High schizophrenic areas [h]	81	25.4	262	74.6	319	48
High manic-depressive areas	103	29.3	249	70.7	352	52

	Church Affiliation							
	Protestant		Catholic		No Church		Totals	
	N	Per Cent	N	Per Cent	N	Per Cent	N	Per Cent
High schizophrenic areas [i]	76	47.2	59	36.6	26	16.2	161	46
Low schizophrenic areas	150	78.1	17	8.9	25	13.0	192	54
High manic-depressive areas [j]	125	71.0	25	14.2	26	14.8	176	64
Low manic-depressive areas	65	65.7	6	6.1	28	28.2	99	36
High schizophrenic areas [k]	76	47.2	59	36.6	26	16.2	161	48
High manic-depressive areas	125	71.0	25	14.2	26	14.8	176	52

	Presently Active in Church					
	Yes		No		Totals	
	N	Per Cent	N	Per Cent	N	Per Cent
High schizophrenic areas [l]	135	83.9	26	16.1	161	46
Low schizophrenic areas	167	87.0	25	13.0	192	54

[a] Chi-square=34.13; P less than .001
[b] Chi-square=6.56; P less than .02
[c] Chi-square insignificant
[d] Chi-square=58.11; P less than .001
[e] Chi-square insignificant
[f] Chi-square=120.23; P less than .001
[g] Chi-square=29.80; P less than .001
[h] Chi-square insignificant
[i] Chi-square=44.23; P less than .001
[j] Chi-square=9.99; P less than .01
[k] Chi-square=25.00; P less than .001
[l] Chi-square insignificant

munities. No difference, however, was found between the high-rate areas in such membership.

The tenth null proposition is rejected, since residents of the high-rate schizophrenic communities had significantly fewer members of lodges or fraternal organizations while no difference was found between the manic-depressive areas.

While residents of both high-rate schizophrenic and manic-depressive communities were found to have voted significantly less in the recent presidential election and a recent city council election, no difference was found between these high-rate areas in their voting behavior. While differences were found between the psychotic communities in church affiliation, no difference was revealed in the amount of active church membership.

This factor, therefore, of low social participation was less supported by the findings than anticipated. It is possible that an extension of the number of voluntary associations from the four realms of participation entailed in this study may reveal more differences in such participation between the differential psychotic communities. From the results of this inquiry, however, only in the realm of lodges and fraternities do inhabitants of high-rate schizophrenic areas exhibit less participation. Whether or not this particular aspect of low social participation is specifically related to the onset of schizophrenia is only conjectural. Low social participation, at least, is apparently related to both high-rate psychotic areas rather than to any particular mental disorder.

Occupational Participation. Two out of the three hypotheses involving this criterion of isolation were rejected, with the high-rate schizophrenic areas found to have less frequent and more sporadic occupational participation, as summarized in Table 4. Not only the high-rate schizophrenic but also the high-rate manic-depressive communities were found to have significantly more unemployed persons than their low-rate counter-areas. A subsequent test showed, however, that the high schizophrenic areas had significantly greater unemployment than the high-rate manic-depressive communities. Inhabitants of the high-rate schizophrenic areas also were found to have held significantly more jobs than the low-rate communities, while no such difference was found between the manic-depressive areas. The fact of ever having quit or been discharged from a job cannot be deemed precipitant specifically of schizophrenia, however, since no significant difference between the high and low-rate schizophrenic communities was found for this characteristic.

Low Frequency of Interaction with Other Areas. This factor seems indicative of schizophrenia as indicated by the rejection of all four of the remaining null propositions, presented in Table 5. Residents of the high-rate schizophrenic communities went downtown significantly fewer times in the four-week period prior to the interviews than did those in the low-rate schizophrenic areas, while occupants of the high-rate manic-depressive neighborhoods showed significantly higher frequency of such trips, in direct opposition to the pattern shown by the high-rate schizophrenic areas. A similar contact between the schizophrenic and manic-depressive areas was found in the number of visits with friends in the previous month. Inhabitants of the high-rate schizophrenic areas showed significantly fewer visits than the low-rate schizophrenic neighborhoods, while those in the high-rate manic-depressive areas visited friends neither more nor less than the low-rate manic-depressive communities.

Residents of the high-rate schizophrenic areas were found also to have visited other areas of the city significantly less than those in the low-rate schizophrenic communities, while no difference was found for the differential manic-depressive neighborhoods. The nineteenth and final null proposition was also rejected when it was found that inhabitants of the high-rate schizophrenic communities traveled out of town significantly fewer times in the previous month than those in the low-rate schizophrenic areas. No difference in this factor was found between the high and low-rate manic-depressive communities.

With the rejection of all four of the null hypotheses concerning the frequency of interaction with other areas by inhabitants of the schizophrenic communities, the inference that low frequency of interaction is

particularly conducive to social isolation and to the precipitation of schizophrenia appears to be consistent with these findings.

The factor of remote location of friends was found to be significant between both psychotic communities, but in an unexpected direction. The results indicate that occupants of both high-rate psychotic areas had their friends living closer by than those in the low-rate communities. This may be a reflection of having fewer friends than those in the low-rate polar-areas. Or it may further indicate social isolation by indicating lack of friends in remote areas of the city, thereby reducing the range or horizon of intimate, primary associations. In either event, the factor of location of friends is not clear in this analysis.

TABLE 4. DIFFERENTIAL OCCUPATIONAL PARTICIPATION BETWEEN SCHIZOPHRENIC AND MANIC-DEPRESSIVE AREAS

	Working Force					
	Unemployed		Employed *		Totals	
	N	Per Cent	N	Per Cent	N	Per Cent
High schizophrenic areas [a]	505	6.5	7270	93.5	7775	49
Low schizophrenic areas	125	1.5	8046	98.5	8171	51
High manic-depressive areas [b]	245	3.2	7511	96.8	7756	59
Low manic-depressive areas	85	1.6	5249	98.4	5334	41
High schizophrenic areas [c]	505	6.5	7270	93.5	7775	50
High manic-depressive areas	245	3.2	7511	96.8	7756	50
	Number of Jobs Held					
	Above Median		Below Median		Totals	
	N	Per Cent	N	Per Cent	N	Per Cent
High schizophrenic areas [d]	86	61.0	55	39.0	141	44
Low schizophrenic areas	74	41.3	105	58.7	179	56
High manic-depressive areas [e]	79	50.6	77	49.4	156	53
Low manic-depressive areas	67	49.3	69	50.7	136	47
	Quit or Fired from Jobs					
	Yes		No		Totals	
	N	Per Cent	N	Per Cent	N	Per Cent
High schizophrenic areas [f]	57	40.7	83	59.3	140	43
Low schizophrenic areas	69	36.9	118	63.1	187	57
High manic-depressive areas [g]	57	33.9	111	66.1	168	55
Low manic-depressive areas	44	31.7	95	68.3	139	45

* Adapted from U. S. Census of Population: 1950, *Census Tract Statistics*, III, Ch. 3.
[a] Chi-square=259.16; P less than .001
[b] Chi-square=31.79; P less than .001
[c] Chi-square=94.06; P less than .001
[d] Chi-square=12.18; P less than .001 (Median=2)
[e] Chi-square insignificant (Median=2)
[f] Chi-square insignificant
[g] Chi-square insignificant

SUMMARY AND CONCLUSIONS

With the rejection of thirteen out of the nineteen initially proposed null propositions concerning elements of social isolation and the differential incidence of schizophrenia in four census tracts of a middle-sized city, the general tenability of the social isolation hypothesis seems warranted. The high-rate schizophrenic communities were found to have more social isolation as indicated by the following factors of their inhabitants:

(1) Knowing the names of fewer neighbors.
(2) Fewer personal friends.
(3) Fewer acquaintances.
(4) More renting than owning of homes.
(5) Less membership in lodges or fraternal organizations.

(6) Greater unemployment.
(7) More job turnover.
(8) Fewer visits to the central business district.
(9) Fewer visits with friends.
(10) Fewer visits to other areas of the city.
(11) Fewer trips out of town.
(12) Less *inter*-city migration.
(13) Fewer friends in remote areas.

The preceding hypotheses and their appropriate factors seem to suggest that a formal index of social isolation can be formed with the above items as its core or foundation. Perhaps with such an index, or even a scale, a more precise measure of the relationship between social isolation and schizophrenia can be obtained. These factors, at least, may serve as the beginning of

TABLE 5. DIFFERENTIAL FREQUENCY OF INTERACTION WITH OTHER AREAS BETWEEN SCHIZOPHRENIC AND MANIC-DEPRESSIVE COMMUNITIES

	Number of Trips Downtown					
	Above Median		Below Median		Totals	
	N	Per Cent	N	Per Cent	N	Per Cent
High schizophrenic areas [a]	42	40.0	63	60.0	105	58
Low schizophrenic areas	49	63.6	28	36.4	77	42
High manic-depressive areas [b]	48	64.9	26	35.1	74	44
Low manic-depressive areas	37	38.5	59	61.5	96	56
High schizophrenic areas [c]	41	38.7	65	61.3	106	59
High manic-depressive areas	49	66.2	25	33.8	74	41
	Number of Visits With Friends					
	Above Median		Below Median		Totals	
	N	Per Cent	N	Per Cent	N	Per Cent
High schizophrenic areas [d]	31	32.0	66	68.0	97	56
Low schizophrenic areas	56	72.7	21	27.3	77	44
High manic-depressive areas [e]	37	50.0	37	50.0	74	45
Low manic-depressive areas	47	50.0	47	50.0	94	55
	Number of Visits to Other Areas of the City					
	Above Median		Below Median		Totals	
	N	Per Cent	N	Per Cent	N	Per Cent
High schizophrenic areas [f]	34	32.4	71	67.6	105	58
Low schizophrenic areas	57	74.0	20	26.0	77	42
High manic-depressive areas [g]	36	50.7	35	49.3	71	43
Low manic-depressive areas	46	49.5	47	50.5	93	57
	Number of Trips Out of Town					
	Above Median		Below Median		Totals	
	N	Per Cent	N	Per Cent	N	Per Cent
High schizophrenic areas [h]	46	43.0	61	57.0	107	58
Low schizophrenic areas	46	59.7	31	40.3	77	42
High manic-depressive areas [i]	33	44.6	41	55.4	74	44
Low manic-depressive areas	51	54.3	43	45.7	94	56

[a] Chi-square=9.92; P less than .01 (Median=4)
[b] Chi-square=5.79; P less than .02 (Median=4)
[c] Chi-square=13.22; P less than .001 Median=4)
[d] Chi-square=28.52; P less than .001 (Median=3)
[e] Chi-square insignificant (Median=4)
[f] Chi-square=30.82; P less than .001 (Median=3)
[g] Chi-square insignificant (Median=4)
[h] Chi-square=5.02; P less than .05 (Median=0)
[i] Chi-square insignificant (Median=0.5)

a search for additional aspects of social isolation that might eventually constitute a valid and more reliable measurement of this phenomenon of social disorganization and its crucial relation to schizophrenia.

Nevertheless, the tenability of the hypothesis relating social isolation to the incidence of schizophrenia by the relatively crude measures at our disposal seems to be indicated. At least the prevalence of a high degree of social isolation in those communities known to have high incidence-rates of this mental disorder has been empirically established, and warrants serious consideration as a precipitating influence in the social etiology of schizophrenia.

SOCIAL MOBILITY AND MENTAL ILLNESS

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The idea that an individual's movement in the social structure is associated with the development of psychiatric difficulties has been expressed both by psychiatrists [1] and sociologists.[2] Some empirical research has been done on the question,[3] but psychiatrists and sociologists have not worked together [4] previously to determine if psychiatric patients are more or less mobile socially than comparable non-patients. The research reported here has attempted to do this.[5] However, before the findings are presented, the methodological procedures involved in the study will be outlined.

RESEARCH DESIGN

This research was designed so that class position, on the one hand, and the presence or absence of diagnosed functional mental illness, on the other, could be controlled. It was reasoned that by holding each of these factors constant, premised interrelationships between social mobility and mental illness could be found. To achieve this objective, sub-samples of psycho-

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[1] Sigmund Freud, *Collected Papers,* Vol. III, No. 4, Psychoanalytic Notes Upon an Autobiographical Account of a Case of Paranoia (Dementia Paranoides), pp. 445 ff; Karen Horney, *The Neurotic Personality of Our Time,* New York: Norton and Co., Inc., 1937, pp. 80–82, 178–179; Abraham Myerson, "Review of *Mental Disorders in Urban Areas,*" *American Journal of Psychiatry,* 96 (January, 1940), pp. 995–997.

[2] Pitirim Sorokin, *Social Mobility,* New York: Harper and Brothers, 1927, especially, pp. 510–511, 515, 522–525; W. L. Warner, "The Society, The Individual and His Mental Disorders," *American Journal of Psychiatry,* 94 (September, 1937), pp. 275–284; R. K. Merton and Alice S. Kitt, "Reference Group Theory and Social Mobility," in R. K. Merton and Paul F. Lazarsfeld, *Studies in the Scope and Method of "The American Soldier,"* Glencoe, Illinois: The Free Press, 1950, pp. 84–88.

[3] J. Ruesch and others, *Chronic Disease and Psychological Invalidism,* New York: American Society for Research in Psychosomatic Problems, 1946; J. Ruesch, Annemarie Jacobson, and Martin B. Loeb, "Acculturation and Illness," *Psychological Monographs: General and Applied,* 62 (1948), Whole No. 292; John Dollard, "The Life History in Community Studies," *American Sociological Review,* 3 (October, 1938), pp. 724–737; John Dollard and Neal Miller, *Personality and Psychotherapy,* New York: McGraw-Hill, 1950; Evelyn Ellis, "Social Psychological Correlates of Upward Social Mobility Among Unmarried Career Women," *American Sociological Review,* 17 (October, 1952), pp. 558–563.

[4] The present research team is composed of two psychiatrists, Drs. F. C. Redlich and B. H. Roberts, and two sociologists, A. B. Hollingshead and J. K. Myers.

[5] For earlier reports on this research see: A. B. Hollingshead and F. C. Redlich, "Social Stratification and Psychiatric Disorders," *American Sociological Review,* 18 (April, 1953), pp. 163–169; F. C. Redlich, A. B. Hollingshead, *et al.,* "Social Structure and Psychiatric Disorders," *American Journal of Psychiatry,* 109 (April, 1953), pp. 729–734; H. A. Robinson, F. C. Redlich and J. K. Myers, "Social Structure and Psychiatric Treatment," *American Journal of Orthopsychiatry* (April, 1954).

neurotic and schizophrenic patients in classes III and V were drawn from the psychiatric population[6] of the New Haven community. Comparable control sub-samples of non-patients from classes III and V were drawn from the general population.[7]

The sub-samples were selected from non-adjacent classes because we believed that the influence of class factors could be determined more easily in persons from distinctly different classes than in persons from adjacent classes. Classes III and V were selected for the following reasons: *first* these classes have sharply different prevalence rates for treated schizophrenia and psychoneurosis; *second,* they have not been studied carefully in previous psychiatric research; and *third,* they comprise approximately 40 per cent of the population of the New Haven community.

Each class may be characterized briefly as follows: Class III is composed of proprietors of small businesses, white-collar workers, and skilled manual workers who are, for the most part, high-school graduates. These people live in apartments, flats, and single family dwellings in widely-scattered residential areas. Class V is composed almost exclusively of unskilled and semi-skilled workers who typically have an elementary education or less and who live in the most crowded slum areas of the city.

The combination of patients from two different diagnostic categories and two social classes, with non-patients from the same classes, produced the six cell research design presented in Table 1. A glance at Table 1 will show that each of the four cells for patients is filled with a minimum of 12 cases; each cell for non-patients is populated with 30 individuals.

All individuals in the study, patients and non-patients, are white and between the ages of 22 and 44. These age limits were imposed because attention was focused upon patients who presumably had reached adult responsibility and adjustment, but who had not entered the involutional period. The ages of the non-patients were held to the same limits so comparisons could be made between the two groups.

Detailed data were collected on each patient by the psychiatrist and sociologists with a 128-page schedule.[8] While the data were being assembled on the patients, the sociologists interviewed the non-patients with a shorter schedule.

The representativeness of the sub-samples of patients and non-patients to their appropriate universes was crucial to the research. This was complicated by the differences in the ways the two sub-samples were obtained. Patients who met the requirements of the research design were selected individually. The non-patients, on the other hand, were selected at random from the 5 per cent systematic sample of the community's population used in earlier phases of the research. Representativeness of the non-patients was determined by comparing them with the systematic sample of the

TABLE 1. NUMBER OF PATIENTS AND NON-PATIENTS STUDIED BY CLASS

	Patients		Non-Patients
Social Classes	Neurotics	Schizophrenics	Controls
III	13	13	30
V	12	12	30
Total	25	25	60

[6] A *Psychiatric Census* of patients in the New Haven community on December 1, 1950, was taken by the Research Team. For a report of this activity, see A. B. Hollingshead and F. C. Redlich, "Social Stratification and Psychiatric Disorders," *op. cit.*, p. 166.

[7] A systematic 5 per cent sample of the population of the New Haven community in November, 1950, was interviewed by the Research Team. This is reported in *ibid.*

[8] The schedule was divided into four parts. The first part was filled out by a psychiatrist in interviews with the patient; the second was filled out by a psychiatrist in an interview with the patient's therapist. The third and fourth parts were filled out by a sociologist in interviews with members of the patient's family of orientation and his family of procreation. In addition, considerable material came from the clinical record and the clinical interview that Dr. Redlich had with each patient at the end of the interviewing process. As a last step, the team developed two assessment schedules to evaluate the data systematically. The schedule covered the following areas: psychopathological history, history of physical illnesses, attitude toward psychiatry and psychiatric treatment, social identification, family dynamics, education, religion, ethnicity, recreation, occupation, housing, and social class.

general population on the following variables: age, sex, religion, ethnic origin, and class score. The patients were compared with the psychoneurotics and schizophrenics in the psychiatric population [9] on the same variables. No significant difference was found at the 5 per cent level of confidence on any variable when the two groups were compared with their parent universes.[10] In short, the patients were representative of all psychiatric patients in their appropriate age, sex, class, and diagnostic groups, and the non-patients were representative of the New Haven population in their age, sex, and class groups. When the representativeness of the two sub-samples was established, assumed interrelations between class position, mobility factors, and mental illness were tested.

The general proposition under study hypothesizes that in the several social classes, interrelationships exist between mobility factors and diagnosed psychoneurosis and schizophrenia. Data on only two types of mobility factors are reported here. They are: (1) achieved social mobility, and (2) discrepancies between an individual's achievements and his aspirations. A specific proposition on each factor was stated as follows:

(1) A differential relationship exists in the amount of social mobility achieved by non-patients in comparison with psychoneurotics, or schizophrenics, in classes III and V.

(2) A significant discrepancy exists between a class III, or a class V, psychoneurotic's or schizophrenic's achievements and his aspirations.

The first proposition grew out of analyses the team had made of different aspects of mobility among 847 schizophrenics in an extensive study reported elsewhere.[11] That work indicated that the originally hypothesized relationship between status striving and mental disorders was probably different in the several classes. The second proposition was formulated after preliminary analysis of the data on mobility indicated interrelationships between the disorders of the psychoneurotic and schizophrenic patients and their efforts to realize their aspirations. Unfortunately, we were unable to make the same analyses in the non-patient group, because the data were not identical for the patients and non-patients.

[9] The patients in the *Psychiatric Census* mentioned in Footnote 6 are referred to here.

[10] Chi-square, the *t-test*, and analysis of variance were used on appropriate variables to determine whether the sub-sample under test varied significantly from its parent universe.

[11] A. B. Hollingshead and F. C. Redlich, "Social Stratification and Schizophrenia," *American Sociological Review*, 19 (June, 1954); A. B. Hollingshead and F. C. Redlich, "Schizophrenia and Social Structure," *American Journal of Psychiatry*, 110 March, 1954), pp. 695–701.

THE FINDINGS

Proposition One: *Achieved Social Mobility*. Statistical tests of the first proposition were dependent upon the measurement of achieved social mobility. It was measured by the use of Hollingshead's two factor *Index of Social Position*. This *Index* is based upon education and occupation. To use it, the number of years of school the individual has completed is scored on an educational scale; likewise, his occupation is scored on an occupational scale.[12] Then, the scale value for education is multiplied by a weight of six, and the scale value for occupation by a weight of eight. The resulting calculated score is assumed to be a measure of the individual's position in the community's class structure.[13]

Two *Index* scores were computed on each patient and non-patient. The first was the score of the individual's parental family; the second was the score of the individual being studied. The difference between the score of the parental family and the score of the individual in the study, whether positive or negative, was used as the measure

[12] The occupational rating used here was the last job the patient held before he entered treatment. His father's occupation was the one he followed in his mature years.

[13] The scores on this *Index* range from 14 to 98. A score of 14 represents the highest position an individual could reach by a combination of outstanding educational and occupational achievements and 98 the lowest position. To receive a score of 14 an individual has to have a graduate professional degree, and be engaged in a profession, or be a high executive in a large business. A score of 98 is assigned to an individual with less than seven years of schooling, who is an unskilled laborer. All degrees of education and types of jobs fall within these extremes.

of the individual's achieved social mobility. If the difference was positive, the individual was considered to be upward mobile; if negative, he was viewed as downward mobile.[14]

When achieved social mobility had been defined, the crucial question was: Have the psychoneurotics or the schizophrenics in either class III or class V been significantly more mobile or less mobile than the non-patients? Answers to this question were sought by making a series of comparisons of social mobility scores of the non-patients with the patients.[15]

Achieved Social Mobility in Class III. Class III individuals, both patients and non-patients, were far more mobile than class V individuals.[16] As we expected, achieved social mobility in class III was almost entirely upward. Only three individuals were downward mobile by as many as 10 points; one was a non-patient, one was a psychoneurotic, and one was a schizophrenic. All others were upward mobile by varying amounts. Three patients moved upward more than 50 points. All were females; one was a psychoneurotic; the others were schizophrenics.

The amounts of mobility achieved by the non-patients, the psychoneurotics, and the schizophrenics, are summarized in Table 2. Table 2 shows that, in comparison with their parental families, the non-patients moved upward 20 points, the psychoneurotics 27 points, and the schizophrenics 36 points on the *Index of Social Position*. The differences in achieved upward mobility between both the psychoneurotics and the non-patients, and the schizophrenics and the non-patients, are striking.[17] These data indicate a definite interrelationship between social mobility and mental illness. The controls have been the least mobile, and the schizophrenics the most mobile of the three groups. This suggests a correlation between the extent of a class III individual's achieved mobility and the severity of his illness.[18]

TABLE 2. MEAN NUMBER OF MOBILITY POINTS ACHIEVED BY THE NON-PATIENTS, THE PSYCHONEUROTICS, AND THE SCHIZOPHRENICS BY CLASS

Group	Points Achieved in Each Class	
	III	V
Non-patients	20	8
Psychoneurotics	27	12
Schizophrenics	36	8

Since the achieved mobility scorces were calculated by comparing the scores of the present generation with the parental generation, the next step was to determine if the the non-patients, the psychoneurotics, and the schizophrenics had the same, or different, parental base lines. Components included in the base line were: (1) the *Index of Social Position* of the family or orientation; (2)

[14] Difference in scores between the parental and the present generation could be a consequence, either of the increased amount of education received by the present generation, or changes in the occupational structure that have occurred over a generation. A third possibility is that they are a combination of both. Although this point is of general interest it is not germane to this discussion, because changes in the educational and occupational areas of the culture have been controlled by the research design.

[15] The *t-test* was relied upon in each comparison to determine significance of the differences between the mobility scores of the control and the patient groups.

[16] This finding was in accord with the general assumption that class III represents one sub-cultural group, and class V a distinctly different one. Comparisons of social mobility between class III and class V controls, schizophrenics, and psychoneurotics produced 2-tailed *t*-test values that were significant beyond the .005 level of confidence.

[17] Differences between (a) the psychoneurotics and the non-patients are significant beyond the 10 per cent level of confidence; (b) the schizophrenics and the non-patients beyond 5 per cent level of confidence. Interpretation of these levels of significance requires a word of explanation. The *t*-test was used to test significance of differences. When this test is used on relatively small samples with large variances the degree of significance that may be expected is limited. Since our samples were small and the variances were large, the significance of the differences stated here is conservative.

[18] When the distinct relationship had been found between achieved social mobility and both psychoneurosis and schizophrenia, social mobility scores were computed on the patients' adult brothers and sisters to see if they had been as mobile as the patients. This operation showed that the psychoneurotic and schizophrenic patients in class III had been significantly more mobile than their siblings. The one-tailed *sign test* was used. Probability was less than 2 per cent for the psychoneurotics, and less than 5 per cent for the schizophrenics in comparison with the median of their adult siblings.

ethnic orgins, and the number of generations in the United States; (3) religious affiliation; and (4) community of origin. No significant differences were found on any of these factors[19] when the non-patients in class III were compared with the psychoneurotics and schizophrenics. Two conclusions were clear from these comparisons: first, that the three groups, non-patients, psychoneurotics, and schizophrenics, had come from an essentially homogeneous social and cultural base; any differences that existed were of a random order; second, that the demonstrated differences in achieved social mobility in the present generation were produced by the differential efforts of the individuals in the study to attain more education and to get better jobs than their parents had had. In addition, the inference may be made that the psychoneurotics, and espically the schizophrenics, were over-achievers.

Achieved Social Mobility in Class V.[20] The average achieved social mobility for each group in class V is summarized also in Table 2. A glance at Table 2 will show that the non-patients, on the average, moved upward 8 points. The schizophrenics also moved upward 8 points, but the psychoneurotics made a 12 point gain. The amount of upward mobility achieved by the psychoneurotics is significantly more than by the non-patients; obviously, there is no difference between the non-patients and the schizophrenics.[21]

The analysis of the data on the family base lines of the class V's showed that there were no significant differences between the patients and the non-patients on ethnic origins and the number of generations in the United States, religion, or community of origin. However, the psychoneurotics came from families with lower scores on the *Index of Social Position* than the non-patients. This difference was not significant, but the tendency was strong. The schizophrenic patients came from a significantly lower base line than the non-patients. In fact, *all* of the schizophrenics came from families with the lowest possible scores on the *Index of Social Position*. This fact needs to be taken into consideration in any interpretation of the amount of achieved mobility among these patients.

The demonstrated relationship between upward mobility and psychoneurosis in both class III and class V, is in accordance with the first proposition; but, the data on the schizophrenics only partially support it. In class III, the schizophrenic patients are significantly more mobile than the non-patients; in class V there are no differences on the amount of mobility achieved by the schizophrenics and the non-patients. However, the data on the patients and the non-patients in class III, and in class V, are consonant with the assumption that achieved social mobility is a function of an individual's position in the class system. Stated otherwise, the data show that the class III individual is in a position to achieve a considerable measure of upward mobility, but the class V individual does not have the same kinds or types of opportunity.

Proposition Two: Discrepancies Between Achievement and Aspiration. The attempt to assess the meaning that education and occupation had for the patients gave rise to the second proposition, regarding meaningful interrelationships between what the patients had achieved and what they had hoped to achieve. Achievement was defined, in accordance with proposition one, as the position a patient had attained educationally and occupationally. What he said he wanted

[19] The *t*-test was used to test significance of difference on the *Index of Social Position* between the families of orientation of the non-patients in comparison with the psychoneurotics, and the schizophrenics; chi-square was used to test significance on the other factors in the base lines.

[20] Achieved social mobility patterns are very different in class V from those in class III. Class V individuals are not as mobile as the class III's, but the general tendency is upward. Surprisingly enough, no class V individual was downward mobile and only one, a schizophrenic, was upward mobile by as many as 20 points. These figures indicate that there is much less variance in class V than in class III.

[21] When the achieved mobility data of the patients were compared with those of their adult brothers and sisters, no significant difference was found between the mobility scores of the psychoneurotics and their siblings. The schizophrenics, however, had significantly lower mobility scores than their brothers and sisters. They appeared to be under-achievers within the sibling group. However, the differences between the schizophrenic patients and their non-patient siblings were small.

to achieve in each area was defined as aspiration. The difference between what was attained and what was hoped for was defined as discrepancy. The discrepancy, if any, between achievement and aspiration was assumed to be a stress vector in the patient's life.

Throughout this phase of the analysis we were concerned with the question: Are the aspirations expressed by these patients an integral part of their personality structures or are they merely scenery on the stages of their make-believe worlds? In order to answer this question, each patient's history was studied to determine if evidence supported his statements of his aspirations. If his pre-morbid behavior indicated he had made more or less consistent efforts to bridge the gap between his claimed aspirations and his actual achievements, it was inferred that verbalized statements of his hopes were meaningful elements in his personality.

Educational Discrepancies—Class III. The data on educational achievement, aspiration, and discrepancies in class III are summarized in Table 3. This tabulation

TABLE 3. MEAN EDUCATIONAL ACHIEVEMENT AND ASPIRATIONS MEASURED IN YEARS OF SCHOOL FOR CLASS III PSYCHONEUROTICS AND SCHIZOPHRENICS

	Years of School	
Educational	Psychoneurotics	Schizophrenics
Achievement	13.3	14.1
Aspiration	16.1	16.4
Discrepancy	2.8	2.3
	$F = 8.688, 1/25$ p less than .01	$F = 6.988, 1/25$ p less than .05

shows that the average class III psychoneurotic completed slightly more than one year of college, but he aspired to a college degree. The average class III schizophrenic completed two years of college, and he too, wanted to finish college. The discrepancy between educational achievement and aspiration among the class III psychoneurotics and schizopherenics is significant.

Twelve of the 13 psychoneurotics in class III were dissatisfied with the amount of education they received. One of the 12 summed up these feelings of educational inadequacy when he said with reference to his hopes for his children's education: "Parents today want their children to have the amount of education that the parents themselves wanted to receive." This man had worked his way through two years of college, and he was eager to see his children realize the hopes he had once held for himself. This man was typical, for all these patients had worked hard to achieve their educations. Moreover, they viewed education as the area of activity that would enable them to realize their goals in life.

In class III, schizophrenics emphasized education more strongly than the psychoneurotics; and they implemented their desires by going to school a year longer, on the average. Every schizophrenic had put forth great personal efforts to obtain his education. He was usually a good student; and he enjoyed school. Typically his problem was to get enough education to prepare him for the job he wanted. He worked upon the premise that if he could get enough education he would get the desired job, then he would be accepted socially, and his problems would be ended. Finally, he looked upon education as a panacea for his personal and social problems.

Apparently these patients did not recognize that factors other than education are involved in the realization of successful aspirations. For example, one patient with an I.Q. of 140 graduated in 1936 from high school at the age of 16, with an "A" average. He intensely desired to enter an Ivy League university, but his family was on relief. He took examinations for the United States Military Academy, but did not pass the physical examination. At this point in his career he had a schizophrenic break with a remission of symptoms. He went to work instead of to college, and his dream of a college education appeared to be but a memory. However, World War II came and he joined the Army. While he was overseas he had another psychiatric break and was hospitalized. After the war he married, had a family, and applied for educational benefits under the G.I. Bill, entered college, but not in the Ivy League, and began upon the realization of his educational dream. He worked nights to supplement his G.I. benefits and to support his family, and completed the work for his bach-

elor's degree in the usual four years. However, he discovered to his dismay, that his college education had not fitted him for a particular job he desired. He was frustrated, angry, and misunderstood. His wife could not understand why he could not step out and command a job commensurate with his education so that she could be supported in the style she had dreamed of in the years he was in college. His parents could not see why their son did not get the kind of job they thought a college man should have. He did not realize that he was ten years older than the usual college graduate, and that employers were more interested in this fact than in his education. In short, this man, like the other class III schizophrenics who had over-aspired and over-achieved in the educational sphere, was not able to consolidate his educational achievement. The net result was excruciating anxiety over his failure to realize his job and status aspirations.

Class V. Educational achievements and aspirations among the class V patients are summarized in Table 4. This tabulation

TABLE 4. MEAN EDUCATIONAL ACHIEVEMENT AND ASPIRATIONS MEASURED IN YEARS OF SCHOOL FOR CLASS V PSYCHONEUROTICS AND SCHIZOPHRENICS

	Years of School	
Educational	Psychoneurotics	Schizophrenics
Achievement	8.9	7.3
Aspiration	13.8	10.6
Discrepancy	4.8	3.3
	$F = 14.158, 1/23$ p less than .01	$F = 6.462, 1/23$ p less than .05

shows that the discrepancy between achievement and aspiration is significant for both the psychoneurotics and the schizophrenics.

The class V psychoneurotics believed they could have had skilled jobs, or clerical positions, if only they had been able to finish high school and had received specialized vocational training. They stated their aspirations largely in terms of a better job, and they looked retrospectively to education to solve their economic problems. The function of education, as they looked back upon it, was to prepare an individual for a good job. A good job meant a higher standard of living, and, if one lived well, what other problems could one have?

The class V schizophrenics all encountered educational frustrations. Most of them were compelled to leave elementary school, at the earliest legal age, by a combination of economic circumstances and parental indifference, if not hostility, toward education. As adults they regretted their lack of an education, and they were aware that they could not improve their positions without more education, but they felt incapable of obtaining it.

Occupational Discrepancies—Class III. When we turn from the educational to the occupational area, we find a definite discrepancy between the actual and the idealized. However, there are no differences in the occupations engaged in by the class III psychoneurotics in comparison with the class III schizophrenics. Both groups have moved in this generation from manual work into work that requires specialized training and reasonably smooth interpersonal relations. The men are employed as clerks, salesmen, and supervisors; the women are employed, or they were before marriage, as secretaries, elementary teachers, nurses, and technicians. Although the occupational achievements of both sexes have been substantial, their aspirations are far above their accomplishments. The men would like to be professionals, or in business for themselves; the women would prefer to be professionals, or married to professional men. The occupational reference groups of the patients include lawyers, doctors, professors, engineers, artists, musicians, and business executives.

Only two class III patients—both female—were satisfied occupationally; one was a psychoneurotic and the other was a schizophrenic. The story of the schizophrenic will be outlined to illustrate how this woman's aspirations were linked with her emotional problems. She struggled, over a seven-year period, to work her way through college and a year of graduate school. She obtained a job as a research assistant as soon as she finished her studies. She liked her job, but she was very dissatisfied with the pay and the fact that she saw books, not people. She was forced, by her low salary, to live with several girls, when she desired an apartment of her own, social life, and male friends. She realized her educational and

occupational goals, but they did not provide her with the things she thoght they would. She became anxiety-ridden when she saw that the social goals she had hoped to attain through her long struggle for an education were beyond her reach. Shortly after she realized that her education and her job did not solve her personal-emotional problems, her ego structure collapsed, and she experienced a psychotic episode.

This woman felt throughout her life that she was handicapped severely by her family background. She aspired to a higher status than the one ascribed to her by her family of orientation, and she struggled to achieve a desired social position through education. In the end, she was trapped by her failure to utilize her educational achievement to solve her emotional needs in the social sphere.

Class V. The class V patients, both psychoneurotics and schizophrenics, were either semi-skilled or unskilled workers. They felt their jobs were unsatisfactory; they worried about how long they would last, the nature of the work, that they did not pay enough to meet the needs of their families, that there was no advancement, that the job carried no status, and so on through a long series of specific irritations.

The jobs they aspired to were relatively modest ones, such as stationary engineer, machinist, a foremanship, clerical work. Significantly, not a single class V patient realized his occupational aspirations. As a group, they were aware of the connection between good jobs, steady jobs, jobs that paid a living wage, and a dreamed-of standard of living. Occupational aspirations were stronger among the class V women, both patients and spouses of patients, than among the men. Apparently, they visualized the connection between education, jobs, and mobility better than the men. About one-half of the men hoped for a steady, semi-skilled factory job; the remainder dreamed of skilled jobs. Their wives, however, wanted more money, shorter hours, higher status jobs for their husbands, and a "better shake for the kids."

DISCUSSION

Vertical mobility has been shown to be a factor of significance in both schizophrenia and psychoneurosis, in the representative samples of two classes of the New Haven population. This does not necessarily mean that mobility is the only, or even the principal, causative factor. Nor is there any information here concerning how this factor may contribute to mental abnormality. It seems clear, however, that the relations between status striving, anxiety, and mental health, deserve further intensive investigation.

SOCIAL STATUS DIFFERENTIALS AND THE RACE ATTITUDES OF NEGROES *

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THIS paper reports some of the more salient findings of the second study of a projected multi-phase investigation of the relationship between social status differentials and attitudes in the realm of intergroup relations.

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The first study,[1] the field-work for which was conducted in Indianapolis during 1950, involved the assessment of the attitudes of Whites of varying socio-economic status toward both Negroes and Whites of varying status. The present study, the data for which were gathered in Indianapolis during 1952, is to some extent a mirror-image of

[1] Frank R. Westie, "Negro-White Status Differentials and Social Distance," *American Sociological Review*, 17 (October, 1952), pp. 550–558.

the first phase. Virtually all relevant variables are the same as in the first investigation. This time, however, we are concerned with the attitudes of Negroes toward Whites rather than with the attitudes of Whites toward Negroes.

The research dealing with the attitudes of minority group members toward the majority makes a rather thin chapter in the annals of sociological research, especially when compared to the volume of research on the other half of the intergroup relations picture.

The reasons for this apparent imbalance are fairly obvious. The inter-group relations area has been traditionally handled within a social pathology framework and this orientation has been conducive not only to the espousal of the values of the investigator, but has also lent itself to the assignment of blame and responsibility to particular individuals and groups for the society's failure to realize the investigator's values. Thus what is known in popular parlance as "the *Negro* problem" becomes in effect, in the parlance of social pathology, a "*White* problem"; the social norms which define in negative terms the attitudes and actions of Whites toward Negroes are seen (quite correctly, we might add) to have had their genesis and perpetuation in white society. Given this viewpoint, plus the fact that most investigators in this area have been concerned with amelioration of conflict and the more speedy realization of democratic ideals, it has been deemed efficient to concentrate our research efforts on the attitudes of the majority. The view has been, in effect, that the problem ultimately resides in the attitudes of the majority and that the cure lies in the changing of these attitudes.[2]

If, however, our purpose is to understand majority-minority relations, not as an instance of social pathology or as an extra-societal appendage, but rather as a part of the larger social system, then the attitudes of minority group members toward the majority become much more important than where the orientation is pathological. If minority-majority relationships are indeed an integral part of the system of social relationships which constitute our society, and if our purpose in studying them is to help fill the gaps in our knowledge of our society, then such relationships must be studied *as social relationships,* which means that they must be considered in their *reciprocal* aspects.[3]

Previous studies of the race attitudes of Negroes have been largely concerned with the attitudes of "Negroes in general" toward "Whites in general." The present study is concerned not only with the attitudes of Negroes toward Whites, but with attitudes of Negroes toward Negroes.[4] Moreover, the Negro respondents are given opportunity to respond to members of these two groups, not simply in terms of race, but also according to the within-group status of the attitude-object. By interviewing Negroes on different levels of the socio-economic hierarchy of the Negro community and assessing their attitudes toward whites and Negroes in occupations of divergent status, we establish the relationship between *social status differentials* and the race attitudes of Negroes. The term "social status differential" refers to the difference between the status of the respondent and status of the attitude object.

The instrument which was used in the study of Whites was employed in the present study of Negroes. Despite the fact that the members of another race were interviewed, it was not necessary to make any alterations in the scales, the attitude

[2] Whether attitudes are mere symptoms or in some sense causes, whether or not it is more efficient to treat attitudes rather than social conditions, are considerations which are not relevant to this paper; and, as far as we can tell, the findings contribute little to arguments on either side.

[3] We might add that it is precisely because most sociological investigators fail to consider such relationships in their reciprocal aspects that the larger part of their empirical research in this area is generally indistinguishable from the product of the psychologist. And until we relate this study to the first one, so are ours. Although the present article simply presents the findings regarding the attitudes of Negroes, these findings are being analyzed in relation to the findings regarding the attitudes of Whites in terms of Bogardus' concept of *social distance differentials.* See E. S. Bogardus, "Social Distance Differential," *Sociology and Social Research,* 32 (May–June, 1948), p. 882.

[4] The findings regarding the attitudes of Negroes toward Negroes are not presented in the present article.

objects in the items, or in any other aspect of the instrument. This was due to the fact that the schedule calls for responses to both Whites and Negroes. Thus, the findings of the first study[5] (of Whites) are directly comparable to those of the present study.

The primary reason for including items calling for responses to Negroes as well as to Whites has to do with our technique of measuring prejudice. This device, referred to as the *Summated Differences Technique*[6] yields a prejudice score which is a function of the difference in response to persons of the same occupation but of different races. For this reason we are able to measure prejudice toward Negroes at the same time as we get at prejudice toward Whites.

The instrument has been described at length in a previous article.[7] We need merely repeat here that the schedule includes four social distance scales which measure (1) *Residential Distance* (degree of residential proximity permitted), (2) *Position Distance* (degree of willingness to have the attitude-object occupy positions of power and prestige), (3) *Physical Distance* (the degree to which respondents are averse to physical contact with the attitude-object), and (4) *Interpersonal Distance* (the degree of proximity permitted in interpersonal interaction). The attitude-objects in the items of these scales are not simply "the Negro" or "a White man," but rather Whites and Negroes in eight different occupations.[8] In the present study we asked Negroes of varying status to respond to "Negro Doctor," "White Doctor," "Negro Ditch Digger," and "White Ditch Digger," *etc.*, in the various areas of social interaction implied by the titles of the scales.

THE SAMPLES

A sample of the Negro population of Indianapolis was interviewed during the summer of 1952. The universe from which the sample was drawn was defined to include all Negro males 21 years of age or over, who are heads of households and who live in blocks without Whites.

Since this study is focused upon the relationship between social status differentials and attitudes, it was necessary that our sample represent persons at the upper and lower reaches of the socio-economic ladder in sufficiently large numbers to permit statistical manipulation. This posed a major sampling problem. We were unable to draw area samples along socio-economic lines, as it was impossible to discover any clear-cut socio-economic status areas within the Negro community.[9] In Indianapolis, as elsewhere, Whites, rather than the Negro's income, determine where Negroes shall live, and the area of Negro residence so segregated tends to be uniformly low from the standpoint of socio-economic status. Although there is some variation in house type and dwelling area within the Negro community, the range of variation is small compared to the range in the White community; in the Negro community it is not uncommon to find living next door to one another persons who occupy positions at the opposite extremes of the occupational ladder.

Because of these characteristics of the Negro community, we decided to draw a single sample that would be sufficiently large to turn up a range of socio-economic types broad enough to permit establishing relationships between status differentials and attitudes. This sample was secured by assigning numbers to all blocks in the Negro community and then drawing a sample of blocks. A list of all households within the blocks was prepared from the Indianapolis City Directory. The households were then numbered and samples were drawn in proportion to the number of households in the block. Tables of random numbers were used in these drawings. Respondents were interviewed in the order drawn.

After a few dozen respondents had been interviewed, it became apparent that a single sample would not yield a sufficiently

[5] Westie, *op. cit.*, pp. 550–558.

[6] Frank R. Westie, "A Technique for the Measurement of Race Attitudes," *American Sociological Review*, 18 (February, 1953), pp. 73–78.

[7] *Ibid.*, pp. 73–78.

[8] The total range of occupations used was as follows: Doctor, Business Executive, Banker, Lawyer, Owner-Manager of a Small Store, Bookkeeper, Machine Operator, and Ditch Digger.

[9] From a map of Indianapolis prepared for the first phase in this research program, it was possible to outline the residential areas in which Negroes are concentrated.

large number of persons of high socio-economic status unless this single sample were extended to prohibitive proportions. We decided, however, to continue interviewing until at least 50 persons of lower socio-economic status had been interviewed.[10]

In order to secure, within the limits of our budget, an adequate number of respondents of higher status, it was necessary to resort to an entirely different kind of sampling procedure from that employed for the lower group. The Upper sample was drawn by preparing lists of persons in the professions, higher status white-collar positions, and higher status business positions. All persons in these lists[11] had to qualify for placement in the first three ranks of the Warner scale.

The interviews took place in the home of the respondent and all were conducted by the same interviewer—who is himself a Negro.

THE FINDINGS

The analyses seek answers to the following questions:

(1) What is the relationship between variations in the socio-economic status of Negroes and the social distance they accord Whites in general?

(2) What effect does the occupational status of the white person (as attitude-object) have upon the Negro's response to him?

(3) If the responses of Negroes are affected by the socio-economic status of the white to whom he responds, is this effect equal for different socio-economic levels of the Negro population?

(4) Are the responses of Negroes equally categorical (or non-categorical, as the case may be) in one kind of interaction as in another, or does the degree to which Negroes respond to "Whites as a type" depend on the kind of interaction involved?

Table 1 compares the Upper and Lower samples with respect to the average distance accorded Whites in eight occupations in the four interaction areas. The following general relationships are indicated:

(1) *There is an inverse relationship between the status of the Negro and the distance he would accord Whites: the higher the status of the Negro, the less the distance expressed toward Whites in general.* Thus, in Table 1, the Upper sample has a mean residential distance score of 2.56 toward White Doctor while the Lower sample has a score of 5.14, yielding a difference of 2.58. A qualification, however, is in order: In the Position Area we find that the Upper sample indicated greater distance than the Lower sample toward low status Whites.

[10] Socio-economic status classifications were made according to the occupational scale developed by W. L. Warner, M. Meeker, and E. Eels, *Social Class in America*, Chicago: Science Research Associates, Inc., 1949, Chapter 8. If a person's rank on this scale was 4 or more, he was considered to be of "lower" socio-economic status. Those with a score of 1 to 3 were assigned to the upper group for purposes of analysis. By the time 55 cases had been secured, we found that our quota of lower status persons had been achieved.

[11] The lists were prepared through interviews with business and professional persons in the Negro community. In addition to the list of business men and the list of higher-ranking white collar persons, separate lists were prepared for each of the following professions and white-collar occupations: Medicine, Dentistry, Social Work, Pharmacy, the Ministry (only persons with formal theological training), Law and Accountancy. A list of Negro teachers was secured from the Indianapolis Directory of Public School Teachers. From the standpoint of sampling accuracy, the list procedure is decidedly inferior to the area procedure used for the first sample. The following sources of error should be made explicit: (1) Upper status persons had greater chance of being included in the sample inasmuch as they were not excluded in the definition of the universe from which the "lower" sample was derived. Actually, however, only four persons in the first three ranks of the Warner scale turned up in the first sample. (2) In the construction of the lists it is quite possible that some occupations which qualify persons for the Upper sample are not listed. However, the percentage of such persons is probably small inasmuch as persons who contributed to the list were asked if they knew of persons of higher status who were not in any of the occupations enumerated. (3) Some of the individual lists themselves are probably not exhaustive. However, the Negro community itself is relatively small, (population 65,000) and concentrated. Moreover, the proportion of persons of higher status is smaller than in the White community; the number of persons in any given profession is quite small. Thus, many professional persons who prepared lists of persons in their own profession were quite confident of the exhaustiveness and accuracy of their lists. At least two persons in each profession prepared lists of their own profession. Moreover, the judges were called upon to look for omissions in the lists of the other occupations. The professional persons were particularly helpful in filling gaps in the business and white-collar lists.

As one would expect, upper status persons are more averse than lower status persons to the idea of skilled and unskilled workers occupying positions of power and prestige in the community. It must also be added that, though the relationship outlined here holds in general, the Upper sample tends to express slightly more distance toward White Ditch Digger than does the Lower sample.

in the Lower sample vary less than the Upper sample in their responses to Whites of varying status. Where variations in the responses of lower status Negroes do occur, however, they tend to be in the direction of less distance toward higher status Whites. For the lower status respondents, the magnitude of the variations is so slight that they must be regarded as categorical in their responses to Whites except in the Position Area.

TABLE 1. MEAN SOCIAL DISTANCE SCORES TOWARD WHITES BY SOCIO-ECONOMIC SAMPLES AND INTERACTION AREAS

		Distance Toward									
Area ***	Negro Sample	White Doctor	White Executive	White Banker	White Lawyer	White Owner-Manager	White Bookkeeper	White Machine Operator	White Ditch Digger	Difference Between Doctor and Ditch Digger	N
I	Upper	2.56 *	3.10	2.98	2.93	3.34	3.37	4.24	6.32	3.76	41
	Lower	5.14	5.27	5.76	5.08	5.25	5.37	5.51	6.12	0.98	51
	Diff.**	2.58	2.17	2.78	2.15	1.91	2.00	1.27	−0.20		
II	Upper	5.83	4.59	4.85	3.85	8.85	8.73	10.59	13.41	9.56	41
	Lower	7.24	5.94	6.73	6.24	8.55	8.84	9.63	11.65	5.41	51
	Diff.**	1.41	1.35	1.88	2.39	−0.30	0.11	−0.96	−1.76		
III	Upper	5.63	5.51	5.39	5.32	5.73	5.44	5.76	6.71	1.08	41
	Lower	6.02	6.02	6.24	5.86	6.31	6.08	6.41	6.29	0.27	51
	Diff.**	0.39	0.51	0.85	0.54	0.58	0.64	0.65	−0.42		
IV	Upper	4.10	4.63	4.22	4.39	4.22	4.85	5.22	6.83	2.73	41
	Lower	5.78	6.24	5.73	5.63	6.37	6.20	6.18	6.86	1.08	51
	Diff.**	1.68	1.61	1.51	1.24	2.15	1.35	0.96	0.03		

* Lower scores indicate less distance.
** Difference between Upper and Lower Samples.
*** I=Residential Area
II=Position Area
III=Physical Area
IV=Interpersonal Area

(2) *Least distance is expressed toward Whites of high status while greatest distance is accorded to the lowest status White.* We find, for example, that the Residential Distance accorded White Doctor by the Upper sample is only 2.56, while the distance toward White Ditch Digger is 6.32. This same trend can be discerned in all areas for both the Upper and Lower samples.

(3) *Upper status Negroes are less categorical in their responses to Whites than are lower status Negroes.* Table 1 reveals that the Upper and Lower samples differ in the degree to which increased status of the White operates to mitigate the distance accorded to Whites. In all areas of interaction, persons

(4) *The responses of Negroes to Whites vary according to the area of interaction in which responses are elicited.* The data in Table 1 indicate that Negroes are not equally rigid in their responses to Whites in the various areas of interaction. The status of the White makes greatest difference to Negroes in the Position Area. Both high and low status Negroes would prefer to have Whites of high rather than low occupational status occupy positions of power and prestige in the community. Both upper and lower status Negroes are most rigid (that is, unaffected by variation in the White's occupational status) in the Physical area, the variations here being unsystematic and statistically insignificant. The findings de-

rived from this scale (which seeks to measure the degree to which respondents are averse to physical contacts with the attitude-object) indicate that Negroes do not respond in terms of conceptions of the relative cleanliness of the various categories of Whites.

Critical ratios were computed between average responses to Whites in occupation extremes, that is, between the mean distance expressed toward White Doctor and the mean distance expressed toward White Ditch Digger. Although these are *distance extremes* operationally defined in terms of actual distance expressed, one will agree that they tend to be status extremes as well. Lawyer rather than Doctor is used as the upper extreme in the case of Position Distance, because least distance was expressed toward Lawyer on this scale.

Table 2 presents the critical ratios of the differences between the extreme means. For the upper sample on the Residential Scale the difference in mean distance scores between White Doctor and White Ditch Digger yields a critical ratio of 6.60, which is significant beyond the .01 level of probability. On the other hand, the critical ratio of 1.44 for the lower sample is not significant at the .05 level. On the Position Scale, the Upper sample's critical ratio of 10.28 and the Lower sample's critical ratio of 5.30 indicate significance beyond the .01 level. On the Interpersonal Scale the Upper sample has a critical ration of 3.10 which is significant beyond the .01 level, while the 1.44 critical ratio for the lower sample is not significant at a satisfactory level. Critical ratios were not computed for the Physical Distance data inasmuch as the differences here appear *a priori* to be too small to be statistically significant.

COMPARISON OF NEGRO OCCUPATIONAL CATEGORIES

In Table 3, which deals with Residential Distance, our respondents are classified into seven occupational categories.[12] This table reveals findings similar to those observed in the sample comparisons. The number 1 category, which includes high ranking professional and white collar persons, expresses the least distance toward Whites in general,

TABLE 2. COMPARISON OF MEAN DISTANCE SCORES TOWARD WHITES IN OCCUPATION EXTREMES FOR UPPER AND LOWER SAMPLES OF NEGRO RESPONDENTS

Area	Negro Sample	White Ditch Digger $\overline{X_1}$	White Doctor $\overline{X_2}$	$\overline{X_1}-\overline{X_2}$	C.R. $(\overline{X_1}-\overline{X_2})$	N
I Residential	Upper	6.32	2.56	3.76	6.60	41
	Lower	6.12	5.14	0.98	1.44	51
II Position	Upper	13.41	3.85 *	9.56	10.28	41
	Lower	11.65	6.24	5.41	5.30	51
III Physical	Upper	6.71	5.63	1.08	....**	41
	Lower	6.29	6.02	0.27	....	51
IV Interpersonal	Upper	6.83	4.10	2.73	3.10	41
	Lower	6.86	5.78	1.08	1.44	51

* Lawyer used as occupation extreme in the Position Area.

** Critical ratios were not computed for the Physical Area inasmuch as the differences are too small to be of statistical significance.

[12] The seven point scale adapted from Alba Edwards by W. L. Warner was used for making the occupational classification of respondents. The lists of occupations included in each of the seven categories are too lengthy to be presented here. We can but remark on some of the occupations included in some of the categories along the continuum: Occupations included in Category 1 are of high status, e.g., doctor, lawyer, owner of a large business, big business executive. Categories 3 and 4 include, among others, those occupations associated with the label "white collar," while categories 6 and 7 include, among others, semi-skilled and unskilled workers, truck drivers, janitors, and service persons. The classifications were made on basis of rather detailed descriptions given by each respondent concerning his job activities. For explicit definitions of the categories used, see W. Lloyd Warner *et al., op. cit.*, pp. 140–141.

TABLE 3. MEAN RESIDENTIAL DISTANCE SCORES TOWARD WHITES BY OCCUPATIONAL RANK OF NEGRO RESPONDENTS

Negro Respondent's Occupational Rank	Distance Toward: White Doctor	White Executive	White Banker	White Lawyer	White Owner-Manager	White Bookkeeper	White Machine Operator	White Ditch Digger	Difference Between Doctor and Ditch Digger	N
1	2.00	2.53	2.47	2.80	3.33	3.40	4.80	7.00	5.00	15
2	2.48	3.29	2.86	2.71	2.90	3.00	3.52	6.00	3.52	21
3	4.60	4.00	5.00	4.20	5.20	4.80	5.60	5.60	1.00	5
4	5.17	3.33	4.67	4.67	3.17	4.50	4.83	5.33	0.16	6
5	5.14	6.29	5.00	5.29	5.29	5.29	4.71	5.71	0.57	7
6	5.20	4.53	6.40	4.73	5.00	5.53	5.00	6.20	1.00	15
7	5.09	5.96	5.87	5.35	5.96	5.52	6.26	6.39	1.30	23
Difference between 1 and 7	3.09	3.43	3.40	2.55	2.63	2.12	1.46	—0.61		
Total										92

while the number 7 category, which is made up, for the most part, of unskilled workers and service persons, tends to express greatest distance. Moreover, as in the sample comparisons, high White status tends to mitigate distance as far as upper status Negroes are concerned, while the status of Whites tends to have relatively little effect on the responses of lower status Negroes. In the other three areas (for which tables are not presented here) the same kinds of relationships as described in the sample comparisons are apparent.

One will notice in Table 3 certain inconsistencies in the mean distance expressed by Occupational categories 3, 4, and 5. We suspected these inconsistencies to be due to the smallness of the N's of these three categories. In order to check on this possibility we grouped the seven categories into a three-fold system which would yield a larger N for the middle range. The grouping is as follows: (1, 2), (3, 4, 5), and (6, 7). When the data are grouped in this manner we find in Table 4, that the relationships indicated by the sample comparisons emerge again, somewhat more clearly than where the seven-fold classification was used.

Although the purpose of this paper has been simply to present some of the empirical relationships indicated by this phase of the overall project, we realize that some readers

TABLE 4. MEAN RESIDENTIAL SCORES TOWARD WHITES BY THREE-FOLD GROUPING ON OCCUPATIONAL RANK OF NEGRO

Negro Respondent's Occupational Rank	Distance Toward: White Doctor	White Executive	White Banker	White Lawyer	White Owner-Manager	White Bookkeeper	White Machine Operator	White Ditch Digger	Difference Between Doctor and Ditch Digger	N
Ranks 1 and 2	2.28	2.97	2.69	2.75	3.08	3.17	4.06	6.42	4.14	36
Ranks 3, 4, 5	5.00	4.67	4.89	4.78	4.56	4.89	5.00	5.56	0.56	18
Ranks 6 and 7	5.13	5.39	6.08	5.11	5.58	5.53	5.76	6.32	1.19	38
Difference between Ranks 1 and 2 and Ranks 6, 7	2.85	2.42	3.39	2.36	2.50	2.36	1.70	—0.10		
Total										92

may be curious concerning how the findings of the present study compare with the findings of the previous study of Whites. We can only point out here that the general relationships indicated by the present study are almost exactly similar to those found in the study of White respondents, although in the case of the White study the trends were more consistent and the response-differences between respondent categories somewhat larger.[13]

[13] A detailed comparison of the two studies is in preparation. The reciprocal relationships derived through this comparison are being interpreted in terms of their meaning for the competition theory of race relations.

ANTIFEMININITY IN MEN

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THE publication of *The Authoritarian Personality*[1] proved a provocative event. Already a large body of psychological research has appeared aimed at clarifying, explicating, and measuring the concepts proposed by Adorno, *et al.;* and the success of these investigations attests to the soundness of insight and judgment of the original work. The present report is concerned with one aspect of authoritarianism, derived directly from some of the basic notions in *The Authoritarian Personality.*

Adorno and his colleagues concluded, on the basis of their extensive and intensive interviews with highly authoritarian subjects, that exaggerated masculinity, defensive in nature, is an integral part of the personality structure of the antidemocratic male. They speak of his boastfulness about traits like determination, energy, independence, decisivness, and will power, and his extreme intolerance of males lacking these qualities. Tendencies in themselves toward softness, dependency, and passivity, although apparent in the interviews, are heatedly denied by the authoritarian males.

Based on these findings, and motivated by the relevance to current political and social trends, this study was designed to identify and measure a constellation of beliefs and attitudes broadly called "antifemininity in men." This is doubtless related to, but not identical with, male supremacy notions. The authoritarians seem to endorse passivity, weakness, subservience, and the like, in women—that is, women "in their place." The questionnaire constructed for the present study will be seen to reflect intolerance for tenderness, passivity, and anything hinting at femininity *in men,* together with admiration for a rough, aggressive, strong-willed supermasculinity.

Thirty items were devised and tentatively considered a scale. These "antifemininity in men" items were interspersed with authoritarianism scale items from the F scale (Forms 45 and 40, *The Authoritarian Personality,* pp. 255–257) plus a number of neutral items. The scales were administered to 95 male students in a freshman psychology course.[2] Subjects were instructed to sign their names, since these data were also used in connection with another study. The sample consists of young, white men from many parts of the nation, mostly upper-middle class. In the analysis of the results, comparing high scoring with low scoring subjects, all items were found to discriminate between the high and low groups in the predicted direction. Five of the original 30 items, however, were discarded as not adequately discriminating, and were felt to add little to the total scale. The remaining 25 items appear below:

(1) It makes me feel uncomfortable to see a show where a man dresses up like a woman or tries to imitate a woman.

[1] T. W. Adorno, Else Frenkel-Brunswik, D. J. Levinson and R. N. Sanford, *The Authoritarian Personality,* New York: Harper and Bros., 1950.

[2] The author wishes to express his gratitude to Mr. George Page and Dr. Jack Vernon for making this part of the study possible. Dr. Harrison G. Gough offered many useful suggestions regarding content and phrasing of these questionnaire items.

(2) A man should not let his decisions be influenced by women.

(3) I despise softness and lack of will power in men.

(4) Men should make the really important decisions in the family.

(5) It irritates me to see a man asking for sympathy.

(6) Every boy should take part in some sports, like football, wrestling, and boxing.

(7) I admire a man who is a real aggressive go-getter.

(8) I immediately dislike a man who doesn't have a firm handshake.

(9) A boy should be made to take some hard knocks in life without getting any help from his parents.

(10) It may sound old-fashioned, but it's true that a man's best friend is his mother.

(11) Self-discipline is a fundamental trait for a man.

(12) I sometimes feel that a man who lets others take advantage of him deserves what he gets.

(13) A man should take the upper hand in dealing with a woman.

(14) I really have little sympathy for a man who gets discouraged easily.

(15) When a man does something he ought to be ready to face the consequences whatever they might be.

(16) No matter how they act on the surface, men are interested in women for only one reason.

(17) Women usually seem to prefer the strong, silent type of man.

(18) I am really disgusted by a weakling who can't handle his liquor.

(19) A man should not give in to his emotions.

(20) A man ought to be aggressive about sex.

(21) A strong person will be able to make up his mind even on the most difficult questions.

(22) The military service is no place for a woman.

(23) It irritates me to see a man who can't stand up for his opinions.

(24) A strong person tends not to show his feelings.

(25) When you get right down to it, a period of military service is really a good thing for any young man.

Subjects were instructed to answer the questionnaire by checking +3, +2, +1, −1, −2, or −3 for each item, as indicating "agree very strongly," "agree pretty much," "agree a little," "disagree a little," "disagree pretty much," and "disagree very strongly," respectively. Scores were obtained for both the authoritarianism (F) scale and the "antifemininity in men" scale for all subjects. A product-moment correlation of r=+.70 was found between the two scales. Although no data are yet available on other groups, it seems likely that the relative homogeneity of the present sample tended, if anything to lower the correlation. The relationship between the variables is further indicated by the mean scores for various groups presented in Table 1.

TABLE 1. MEAN ANTIFEMININITY IN MEN SCORES FOR HIGH, MIDDLE, AND LOW AUTHORITARIANS

	N	F Scale Mean	Anti-femininity Mean
High F group	30	+10.87	+20.03
Middle F group	35	−13.28	+ 6.54
Low F group	30	−35.87	−11.33
Total sample	95	−12.79 ±20.34	+ 5.16 ±18.26

No claim is made for the excellence of the questionaire items constructed for this investigation; some could doubtless be made to operate more efficiently by rephrasing, and the reader may feel that important aspects of "antifemininity in men" have been overlooked. Nonetheless, the correlation of +.70 indicates a strong relationship between attitudes toward authority and attitudes toward feminine tendencies in men as deduced from the psychodynamic personality model in *The Authoritarian Personality*.

In discussions of the major example of institutionalized authoritarianism—Nazi Germany—paranoid projection is often invoked. Denial of personal guilt, scapegoating moralistic self-aggrandizement, and contempt for the deviant are involved. Fromm [3] speaks of the sado-masochistic authoritarian character who shows excessive deference to all-powerful leaders coupled with persecution of "inferiors." The strong need for external guides to behavior reflects a vaguely felt

[3] Erich Fromm, *Escape from Freedom*, New York: Rinehart and Company, Inc., 1941.

sense of loss of control of the self, the fear that without inflexible rules one would be at the mercy of pressing wicked impulses. Freud postulated the operation of projection as the defense mechanism in paranoia, and specifically projection of unacceptable feminine impulses (tendencies to latent homosexuality). Accompanying this process is the fear and hatred of others who now seem to embody these (projected) ego-alien characteristics. Others, especially these who awaken passive homosexual feelings and fantasies, are felt to be threats, as indeed they are to the latent homosexual male who is struggling the repress these aspects of himself.

Authoritarianism emerges as a pervasive condition involving strong minority group prejudices, rigidity, narrowmindedness, anti-intellectualism, political reaction, and deference to authority.[4] As revealed in the present study, the authoritarians' adulation of rugged, strong-willed, inflexible supermasculinity and their accompanying contempt for men who fall short of a virile ideal offers support for the thesis that a paranoid-like sexual conflict may be fundamental in authoritarianism.

[4] See, for example, Else Frenkel-Brunswik, "Intolerance of Ambiguity," *Journal of Personality*, 18 (1949), pp. 108–143; A. H. Maslow, "The Authoritarian Character Structure," *Journal of Social Psychology*, 18 (1943), pp. 401–411; M. Rokeach, "'Narrow-Mindedness' and Personality," *Journal of Personality*, 20 (1951), pp. 234–251; R. Meresko, M. Rubin, F. C. Shontz, and W. R. Morrow, "Rigidity of Attitudes Regarding Personal Habits and Its Ideological Correlates," *Journal of Abnormal and Social Psychology*, 49 (1954), pp. 89–93.

THE COMPARATIVE ROLE OF CHILDREN AND ADULTS IN PROPAGANDA DIFFUSION

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WHEN a propagandist sets out to persuade people to adopt his conclusions, he faces the following elementary proposition: contact precedes but does not guarantee communication. That is, before there is a transfer of meaning, or before comprehension and acceptance of a set of symbols can take place, the symbols must come to the attention of the audience in question. In these days of competing global propaganda efforts, even contact is not always readily achieved. The propagandist often has the task of transmitting a vital message to a mass population outside the normal channels of mass communication. The free flow of ideas may be restricted through the rigid control of publication facilities, the jamming of foreign radio broadcasts, the use of closed circuit broadcast systems, or the lack of mass media facilities in many underdeveloped parts of the world. Confronted with such a disposition of normal channels, the propagandist may turn to the mass diffusion of a message through the use of airborne leaflets.[1] The present paper reports an investigation of the process of persuasion via this medium.[2] While leaflet transmission is unique in many respects, the leaflet operator confronts the same basic problems and has available the same basic principles in achieving effective communication as do all communicators. The scope of the general problem is characterized by

[1] The use of leaflets during wartime has been widely publicized. See, for example, Martin F. Herz, "Some Psychological Lessons from Leaflet Propaganda in World War II," *Public Opinion Quarterly* (Fall, 1949), pp. 471–486.

[2] This research was supported, in part, by the United States Air Force under contract AF 33(038)—27522 monitored by the Human Resources Research Institute, Maxwell Air Force Base, Alabama. For other reports from this contract, see Stuart C. Dodd, "Testing Message Diffusion from Person to Person," *Public Opinion Quarterly* (Spring, 1952), pp. 247–262; and Stuart C. Dodd, "Testing Message Diffusion in Controlled Experiments," *American Sociological Review*, 18 (August, 1953), pp. 410–416.

the query, how can we gain attention with, stimulate comprehension of, and insure compliance to a given message?

In achieving contact with as many persons as possible in a target population, the propagandist must rely on considerable social interaction. That is, he cannot expect that everyone, or even a majority of persons, will come in direct contact with the message, as when they pick up a leaflet or directly hear a radio program. In fact, the message may have greater impact and influence if it is widely spread through interpersonal channels. The purpose of this report is to examine the general character of these interpersonal channels with particular reference to the comparative role of children and adults in the learning of a leaflet message and their compliance with its content.

SOURCE OF THE DATA

In its studies of various factors in message diffusion, *Project Revere*, an Air Force research program at the Washington Public Opinion Laboratory, dropped from aircraft some 780,000 leaflets on more than thirty target communities varying in population from about 500 to over 325,000.[3] Following each of these tests, ground observers in the target communities invariably called attention to the active role of children in quickly collecting the leaflets from the ground and circulating them to others throughout the community. School activities were frequently interrupted as children deserted classrooms and school grounds to collect leaflets. On several occasions children even attempted to sell leaflets to observers and to interviewers from the project.[4] The active role of children was thus suggested through informal observation. This development was deemed important enough to warrant more systematic observation.

[3] For reports on these tests, see Melvin L. De Fleur and Edith D. Rainboth, "Testing Message Diffusion in Four Communities," *American Sociological Review*, 17 (December, 1952), pp. 734–737. Also, Ørjar Øyen and Melvin L. De Fleur, "The Spatial Diffusion of An Airborne Leaflet Message," *American Journal of Sociology*, LIX (September, 1953), pp. 144–149.

[4] A popular account of this as it took place in Project Revere's Salt Lake City test can be found in "Postcards from Heaven," *Newsweek*, (August 13, 1951), pp. 70–71.

The reason the investigators were particularly interested in the behavior of children stems from the usual structure of social control. Frequently, during wartime, severe restrictions against handling leaflets are levied against adults in certain target areas. These restrictions obviously reduce the communicative effectiveness of leaflets. On the other hand, children have seldom been subject to equally severe restraints. Thus it may be possible to inform the parents through the children, providing children are capable of learning and retelling the content of propaganda messages. For example, it has been the practice of American military forces to drop leaflets warning civilian populations in enemy industrial cities about forthcoming bombing raids. Such warnings serve several purposes. They provide objective evidence of the humanitarian policies of the United States, they prevent needless loss of life, and they usually reduce war production when workers leave town rapidly for safety. The leaflet warning usually occurs several days in advance of the announced bombing raid. To combat industrial setbacks, enemy leaders have at times prescribed the death penalty for persons apprehended with leaflets. However, even in severely controlled areas, if penalties were extracted from children for leaflet activity they were considerably less severe. Children, then, offer a potential channel through which the propagandist can reach an otherwise unreachable primary audience.

To provide for the systematic observation of message diffusion, a study was designed involving eight small towns in western Washington State. These communities varied in population size from 1,015 to 1,800, but were essentially comparable in cultural, economic, and geographic characteristics. A total of 78,069 leaflets were dropped on the eight towns from small airplanes flying at altitudes around 500 feet. In every town the leaflet drops commenced at approximately 12:30 p.m. on a Wednesday, and the leaflets were spread over the entire city as evenly as possible.

The exact number of leaflets dropped on a given town was determined by the size of its population and by an assigned leaflet ratio. Eight such ratios were employed, one for each town, starting at the lower extreme

with one leaflet for every four persons in the population, and doubling in a regular series to an upper limit of thirty-two leaflets per person. The assignment of the leaflet ratios to the various target towns followed a random procedure, and resulted in from 325 to 37,600 leaflets being dropped on the test communities.

The leaflet used was a 5 x 8-inch yellow card with black letters. The message warned the reader that "One raid by an enemy bomber" could paralyze the normal means of communication—radios, telephones, newspapers—and necessitate the use of leaflets to impart official information. The reader was then asked to do three things: (1) to *fill out* the post card attached to the leaflet, which asked six brief questions, (2) to *mail in* the post card, postage free, and (3) to *pass on* extra leaflets to others. The leaflets also stated the importance of "spreading vital information to everyone" and asked the question, "Did you talk about the leaflets with anyone?" thus stressing to the reader the importance of talking about the leaflet with others in his community.

The major assessment of the response and amount of communication achieved by the leaflet was made by teams of interviewers in face-to-face interviews using a standard questionnaire. A representative sample of the population in each target community (one person, of school age or over, randomly selected from every other household) was interviewed. The respondents ranged in age from five years to sixty-five and over. The median age for all respondents was 40.7 years. With the available information it was possible to classify a given respondent as a child (16 years of age or younger) or as an adult, and to classify his responses according to the size of his family. A total of 1,597 respondents were interviewed.

The questionnaire posed eleven basic queries regarding the respondent's contact with the leaflet and his subsequent behavior about the message content. Those respondents who first learned the message by picking up the leaflet contributed to the *physical diffusion* of the message. Those people who received a leaflet from others, or heard the message orally, contributed to the *social diffusion* of the message. In each of the eight communities the interviewing was started at approximately 9:00 a.m. on the first Saturday following the Wednesday leaflet drop. By prearrangement, the mass media (newspapers, radio and television broadcasters) refrained from mentioning the leaflet drops during the three-day period, so that the message diffusion remained uninfluenced by mass media publicity and may be attributed to the leaflet itself.

ACCURACY OF MESSAGE RECALL

We shall refer to various parts of the population interviewed, and the following summary is presented to help the reader keep these subpopulations in mind:

Total number of interviews	1,597
Children	225
Adults	1,372
Total number of message-knowers	858
Learned by social diffusion	451
Oral	87
Pass-on	364
Learned by physical diffusion	407

One population of primary interest was the total number of *message knowers* in the eight towns. This group can be classified in various ways in order to characterize the communications network operating in the diffusion of the leaflet message. Of the 1,597 respondents, a little more than half, 54 per cent, were identified as message knowers. This classification was made on the basis of a content analysis of the verbatim responses of the interviewees to the question: "What did the leaflet say?" That is, the interviewers recorded the exact words of the respondents' replies to this query, and these recorded statements were examined by a set of judges. If at least one "earmark" (important idea from the leaflet message) was judged to be present, the respondent was classified as a *knower*. Those with doubtful replies concerning the message content, or those with incorrect replies, were added to those who said they did not know anything about the leaflet and were classified as *non-knowers*.

It was possible, however, to estimate more precisely the *degree of communication* achieved by subjecting the responses of the 858 message-knowers to a second more rigorous content analysis. A six-category accuracy scale was devised. The most accurate cate-

gory included responses which contained six earmarks from the leaflet, three of which indicated what the respondent was asked to do ("fill it out," "mail it in," "pass it on") and three of which indicated the respondent knew *why* he was to do it. The least accurate response on this six-point scale indicated that the respondent did not know any of the *what* or *why* items but did clearly express other miscellaneous items or ideas contained on the leaflet (and which could not have been learned from some other source).

Two content analysts, working independently, classified the responses of the 858 message-knowers by the six-point accuracy rating scale. High agreement (96 per cent) was achieved between their judgments. The results of the content analysis, with the median age of the respondents classified for each scale category, are presented in Table 1.

TABLE 1. THE ACCURACY OF LEAFLET MESSAGE RECALL AND THE MEDIAN AGE OF RESPONDENTS IN EIGHT TARGET TOWNS

Accuracy Rating		Number of Knowers	Per cent of Total Knowers	Median Age
Total		858	100	
(Most accurate)	1	38	04	30
	2	145	17	33
	3	116	14	35
	4	216	25	35
	5	286	33	41
(Least accurate)	6	35	04	42
(Leaflet in hand when interviewed)		22	03	64

It may be seen from Table 1 that about half the total target population knew something about the leaflet message. There was, however, considerable variation in their accuracy of recall. For example, only 4 per cent of the knowers knew the *what* and the *why* of the message in complete detail. Also, more than half of the knowers could recall less than half of the message content. In general, there was a tendency for *older* respondents in the population to recall the leaflet message *less accurately;* (though not reported in Table 1, this was equally true for both male and female respondents). Finally, there was some suggestion that the very aged respondents in the population were more apt to have retained a leaflet which was produced when they were interviewed three days after the drop.

Having staked out the boundaries of message-knowing more precisely, we turn to a consideration of how the various age groups became message-knowers, how informer and informed were related, and what they did with the message once they learned it. For these purposes, we shall use the most liberal criterion of message-knowing. That is, we will analyze the communications behavior of the 858 respondents who knew at least one earmark of the leaflet message.

SOCIAL RELATIONSHIP OF INFORMER TO INFORMED

What proportion of the combined target population learned the message by *social* diffusion? This inquiry may be answered in two ways. The 451 respondents who learned the message by social diffusion represent, first of all, 28 per cent of the total interviewed population (N=1,597). Or, they also represent 53 per cent of the message-knowers (N=858).

Using the total interviewed population as the base, social diffusion varied from 15.2 per cent where the fewest leaflets were dropped, to 38.5 per cent where the most leaflets were dropped.[5] To what extent did this social diffusion take place between members of the same household, that is, from husband to wife, child to parent, etc.? For all eight target towns combined, significantly more social diffusion took place between persons from different households, than between persons from the same household. That is, social diffusion outside of household groups to social diffusion inside of household groups took place in a ratio of about six to four. Table 2 presents the findings for the eight towns separately and combined.

Under the conditions specified in this test it is clear that considerable social diffusion took place beyond the mere interaction of members of the same household.[6] However,

[5] The corresponding range in *physical* diffusion was 10 to 49.9 per cent.

[6] This general finding apparently is not peculiar to the small towns of the present study. Similar results were found in Project Revere, Test 7, Number U:53–182. In this test 325,000 leaflets (one leaflet per person) were dropped on Birmingham, Alabama. The leaflets contained the same message in both tests. However, in Test 7 face-to-face

TABLE 2. SOCIAL DIFFUSION INSIDE AND OUTSIDE HOUSEHOLD GROUPS

Target Town and Leaflets per Person		Respondents Learning by Social Means	Inside Household	Outside Household	Difference Divided by the Standard Error of the Difference
Town	Ratio	Number	Per cent	Per cent	
Total		451	40.4	59.6	−8.31*
A	1/4	35	34.3	65.7	−3.92*
B	1/2	39	51.4	48.6	+0.36
C	1/1	33	24.2	75.8	−6.45
D	2/1	80	37.5	62.5	−4.62*
E	4/1	48	41.6	58.4	−2.36*
F	8/1	102	48.0	52.0	−0.81
G	16/1	44	43.2	56.8	−1.82
H	32/1	70	34.2	65.8	−5.57*

*Indicates difference significant beyond .05 level of confidence.

further analysis of the social diffusion channels outside the household indicates that the social interaction did not involve people far removed from each other either in physical or in social distance. In half of the eight towns not one person learned the message from a "stranger" (defined as someone the respondent had never met before). In the remaining towns the diffusion of the leaflet message between strangers never accounted for more than 4 per cent of the social diffusion. In fact, most of the social diffusion took place between neighbors who lived within one block of each other, or between friends who lived within a few blocks of one another in the same community.

interviews were not conducted. The measure of response was by mail-back of the postcard attached to the leaflet. A total of 10,012 leaflets were mailed back. Of these, 4,470 or 44.7 per cent were involved in social diffusion. Of these, in turn, 38 per cent involved interaction between members of the same household, and 57 per cent involved interaction between members of various households. This compares very closely with the results reported above for the eight small towns where 40.4 per cent of the social diffusion involved interaction between members of the same household, and 59.6 per cent involved inter-household interaction.

COMPARATIVE MEANS OF MESSAGE LEARNING BY CHILDREN AND ADULTS

The general nature of the interpersonal communication channels used in social diffusion have been described. We may now turn more specifically to the mechanics of social diffusion by examining how children and adults learn the leaflet message. First, *how many* children and adults in the combined target towns learned the message? The interview estimates of these numbers are presented in Table 3.

Children represent about one in five of the total number of persons interviewed. Compared with adults, a significantly greater percentage of the children knew the message (72 versus 50.7 per cent; P is less than .001). The degree of association between age status and message knowing may be stated in terms of phi correlation as equal to —.46.

We have noted that more than half of the 858 message-knowers (53 per cent or 541 cases) learned the message either by hearing it from someone or receiving a passed-on leaflet. In Table 4 the nature of this social diffusion is further analyzed. About one in five cases of social diffusion involved the

TABLE 3. MESSAGE-KNOWERS BY CHILD AND ADULT AGE STATUS

	Age Status					
	Children		Adults		Totals	
	Number	Per cent	Number	Per cent	Number	Per cent
Totals	225	100.0	1372	100.0	1597	100.0
Knowers	162	72.0	696	50.7	858	54.0
Non-Knowers	63	28.0	676	49.3	739	46.0

TABLE 4. CHANNEL OF SOCIAL DIFFUSION IN THE MESSAGE-LEARNING OF CHILDREN AND ADULTS

Channel of Social Diffusion		Age Status			
		Adults		Children	
		Number	Per cent	Number	Per cent
Total	451	369	100	82	100
Oral	87	59	16	28	34
Pass-on	364	310	84	54	66

NOTE: For one degree of freedom, chi-square is 14.1, P is less than .001, and phi-correlation equals .18.

pure *oral* transmission of the message. Again, a significantly greater percentage of children than adults were involved (34 per cent of the children and 16 per cent of the adults; P is less than .001). Conversely, a significantly greater proportion of the adult social diffusion involved receiving a passed-on leaflet (P is less than .001). Thus a child was more apt to *hear* about the leaflet message, and an adult was more apt to have a leaflet *passed on* to him.

From what *source* do children and adults receive their passed-on leaflets or hear the leaflet message? Are adults more apt to receive a leaflet and hear the message from other adults or from children? How does this vary with the number of leaflets that are available in the target community? Some answers to these questions are suggested in Tables 5 and 6 where the amount and direction of interaction between children and adults in high and low leaflet-ratio towns is summarized.

The *source* of a passed-on propaganda message can be assumed to be important when: (1) the bearer comments favorably or unfavorably on the content as he passes it on to others, and (2) the impact of the comment varies with the differential age status of the interacting parties.[7]

Unfortunately in the present instance we do not have any direct evidence on how the message-passer evaluated the content as he passed it on. We suspect that adults, as contrasted with children, have a more structured response either negatively or positively as they engage in social diffusion. This may be so for a number of reasons: (1) Adults generally are guided by a wider range of experience and probably possess more verbal skills for the task of comprehending the leaflet mes-

[7] That older persons tend to be more influential than younger ones in many communication situations has been suggested by a number of studies. See, for example: R. W. Berenda, *The Influence of the Group on the Judgments of Children,* Columbia University, 1950; K. Duncker, "Experimental Modification of Children's Food Preferences Through Social Suggestion," *Journal of Abnormal and Social Psychology,* 33 (1938), pp. 489–507; R. K. Merton, "Patterns of Influence and of Communications Behavior in a Local Community," in P. F. Lazarsfeld and F. N. Stanton, editors, *Communications Research,* 1948–1949, New York: Harper and Brothers, 1949, pp. 180–219; and F. A. Stewart, "A Sociometric Study of Influence in Southtown," *Sociometry,* 10 (1947), pp. 11–31.

TABLE 5. CHILD-ADULT INTERACTION: THE SOURCE OF SOCIAL DIFFUSION IN LOW-RATIO TOWNS

Leaflet or Message Source	Leaflet or Message Recipient					
	Adults		Children		Total	
	Number	Per cent	Number	Per cent	Number	Per cent
Total	116	100.0	29	100.0	145	100.0
Adults	52	44.7	3	10.3	55	37.9
Children	64	55.3	26	89.7	90	62.1

NOTE: For one degree of freedom, chi-square is 11.3, P is less than .001, and phi-correlation equals .28.

TABLE 6. CHILD-ADULT INTERACTION: THE SOURCE OF SOCIAL DIFFUSION IN HIGH-RATIO TOWNS

Leaflet or Message Source	Leaflet or Message Recipient					
	Adults		Children		Total	
	Number	Per cent	Number	Per cent	Number	Per cent
Total	194	100.0	25	100.0	219	100.0
Adults	94	48.5	2	8.0	96	43.8
Children	100	51.5	23	92.0	123	56.2

NOTE: For one degree of freedom, chi-square is 14.8, P is less than .001, and phi-correlation equals .26.

sage; (2) most propaganda messages ultimately require the decision of adults in the target community; and (3) the social status of adults is more clearly crystallized and recognized in the community. These conditions may indirectly influence the reception of the message in transmission and color its interpretation by a receiving adult. Children, on the other hand, are probably more apt to be *neutral transmitters* in the social diffusion process. To the extent that this is true, the impact of the message will neither be strengthened nor weakened when children dominate the process of social diffusion.

Tables 5 and 6 indicate that there is little variation in the pattern of social interaction regardless of the number of leaflets dropped on the target. That is, in both high and low leaflet ratio towns, more of the total leaflet passing and message telling can be attributed to children than to adults (significant at the one per cent level). As indicated in the correlation figure stated below each table, there is a moderate positive tendency for children to interact with children and adults to interact with adults. However, one significant difference concerning these interactions may be noted. When there are *few* leaflets available (low ratio towns), adults receive more of their leaflets from children than from other adults (significant at the five per cent level). When there are *many* leaflets available (high ratio towns), an adult is as apt to receive a leaflet from another adult as he is from a child. Children, on the other hand, report receiving their passed-on leaflets from other children to a greater degree than from adults, regardless of the number of leaflets available (significant at the one per cent level).

COMPARISON WITH A LARGE CITY

To what extent might these findings hold under similar test conditions in a *large city?* We may get some indication of the generality of social diffusion patterns by comparing the results above with those found in another study.[8] A total of 325,000 leaflets (one for every person in the population) bearing the same content were dropped on Birmingham, Alabama. The drop pattern, the day of the week, and the time of the day for the drop were exactly the same in both tests. The major difference in the tests was the way of

[8] This study dealing with ecological factors in message diffusion is reported in full by J. Gordon Shaw in a Technical Research Report at the Washington Public Opinion Laboratory, Document Number U:53–182.

TABLE 7. CHILD-ADULT INTERACTION: THE SOURCE OF PASSED-ON LEAFLETS IN A LARGE CITY (Birmingham, Alabama)

Leaflet Source	Leaflet Recipient					
	Adults		Children		Total	
	Number	Per cent	Number	Per cent	Number	Per cent
Total	2968	100	885	100	3953	100
Adults	1577	53	162	18	1739	44
Children	1391	47	723	82	2114	56

NOTE: For one degree of freedom, chi-square is 341.7, P is less than .001, and phi-correlation equals .29.

measuring the source of passed-on leaflets. In the eight-town study the results are based on *personal interviews*. The Birmingham data reported in Table 7 are tabulations of the *mailed-back* leaflets involved in social diffusion.

In comparing social diffusion in the large city (Table 7) with social diffusion in the small towns (Tables 5 and 6), we get a definite indication that the general pattern of interaction involving children and adults is similar, regardless of city size. In Birmingham, as well as in the eight small towns, children accounted for more leaflet passing than did adults (significant at the one per cent level). In addition, children, to a very high degree, are more apt to receive leaflets from other children in both places. One difference in the passing-on behavior may be noted. Adults receive slightly, but significantly (at the one per cent level) more leaflets from adults than from children in the large city. This is a reversal of the finding from the small towns and suggests the hypothesis that adult-to-adult interaction is relatively more important in large cities, whereas children are more apt to interact with adults in small towns.

COMPARATIVE COMPLIANCE OF CHILDREN AND ADULTS WITH THE LEAFLET MESSAGE

We have examined the ways in which the respondents claimed to have learned the leaflet message. We may now turn to a consideration of how they reacted to the message after they learned it. That is, we are interested in the claimed compliance of adults and children with respect to their *mailing, talking,* and *passing-on* behavior. The latter two acts may be examined as they took place in the eight small towns and in Birmingham. The findings are summarized in Table 8. As all the Birmingham results are based on mailed-back cards, no percentages are reported on mailing compliance. The slightly varying N's on which the proportions for *talking* and *passing* in Birmingham are based are due to variation in the completeness of response in mailed-back cards.

It is clear from Table 8 that the pattern of compliance to the leaflet message was strikingly similar in the small towns and in the large city. In both places children claim to talk about and pass on the leaflets to a significantly greater extent than do adults. The children in the small towns are slightly more active than the big city children, whereas the adult pattern was very similar in both places.

There was no significant difference in the *mailing behavior* of children and adults in the eight small towns. The magnitude of response to the claimed mailing behavior in this case is also very comparable to findings of other studies. For example, in Project Revere's Salt Lake City study 30 per cent of the adults interviewed by telephone, and 30 per cent of the adults interviewed face-to-face, claimed to have mailed in the message.[9] Thus the finding that about *one in three* of the persons in the target area will claim to have complied with the leaflet message requesting a mail-back response appears to be a very stable one.

The high degree of talking compliance reported in Table 8 may indicate a high interest level in these communities with regard to the leaflet. It should be emphasized, however, that we do not know how much of the talking resulted in new message-knowers in the communities. Not only did most of the respondents claim to have talked to someone about the leaflets, but they often claimed to have

[9] Otto N. Larsen, "The Comparative Validity of Telephone and Face-to-Face Interviews," *American Sociological Review,* 17 (August, 1952), pp. 471–476.

TABLE 8. COMPARATIVE COMPLIANCE WITH THE LEAFLET MESSAGE BY ADULTS AND CHILDREN

Compliance Behavior	Eight Small Towns		Birmingham, Alabama	
	Adults (N=696) Per cent	Children (N=162) Per cent	Adults (Talking) (N=3251) (Passing) (N=3192) Per cent	Children (N=921) (N=890) Per cent
Mailing	30.0	28.4	....	....
Talking	77.9	89.5	76.0	82.6
Passing	18.9	48.1	19.0	35.4

talked to several persons. The frequency pattern is reflected in the following figures:

	Median Number of Persons Talked to	
	Eight Towns	Birmingham
Children	5.0	4.4
Adults	3.0	3.6

Thus, not only did a greater proportion of children claim to have talked to others about the leaflet, but they also claimed to have talked, on the average, to more people, both in the small towns and in the large city.

SUMMARY OF FINDINGS

With regard to knowing the message, we may summairze as follows: In the combined eight towns slightly more than half of the interviewed population knew the leaflet message three days after the drop. Children were more apt to be message-knowers than were adults. The degree of message knowing, however, varied considerably. Only four per cent of all the knowers could be scored as *accurately* knowing the entire essential content of the leaflet. More than half of the knowers knew *less than half* of the leaflet content. Younger message knowers tended to know more of the leaflet content than older message knowers. While age made a difference in the degree of leaflet content known, sex did not make a difference. Males and females tended to know the leaflet content equally well at a given age level.

With regard to how the message was learned, we may summarize as follows: social diffusion accounted for slightly more than half of the message-knowing in the combined eight towns. Significantly more social diffusion took place between members of separate households than between members of the same household. (Similar results were found in Birmingham.) Most of the social diffusion took place between friends and neighbors who lived within one block of each other, while the diffusion between "strangers" (someone the respondents had never met before) was negligible, never accounting for more than four per cent of the social diffusion in any given target town. Over-all, there was proportionally more social diffusion in those target towns where relatively few leaflets were available than in those towns where a great volume of leaflets were dropped.

A child was more apt to learn the leaflet message through *oral* diffusion than was an adult. Children received passed-on leaflets almost exclusively from other children in both the small towns and the large city test.

Adults received passed-on leaflets in approximately equal proportions from adults and children in the small towns. However, in the large city, adults received significantly more leaflets from other adults. The amount of social diffusion will vary with the number of leaflets available in a target community, but the pattern of child-adult interaction remains relatively constant regardless of the number of the leaflets.

With regard to complying with the message, we may summarize as follows: Proportionally more children than adults engage in passing on leaflets to others in both the small towns and the large city test. Likewise, proportionally more children than adults claimed to have engaged in conversation about the leaflet with others. In these conversations, the average child claimed to have talked to more people about the leaflet than did the average adult in both the small towns and the large city test.

About one person in every three interviewed claimed to have mailed back a leaflet. (This finding was supported by Project Revere's Salt Lake City study.) There was no significant difference in the proportionate mailing response of children and adults in the small towns.

DISCUSSION

This investigation has been concerned with certain factors operating in the social diffusion of a propaganda message. Social diffusion involves the exchange of a message between two or more persons in a specified population. Under most conditions, not everyone in a target community will have direct access to a message as it is transmitted by the medium. For this reason, the success of propaganda operations can depend largely on how much social diffusion is stimulated.

The degree of social diffusion achieved will undoubtedly vary with many circumstances including the nature of the message content; the composition, mobility, and other characteristics of the population; and with the stress context of the occasion (such as wartime conditions, the imposition of legal re-

strictions, etc.). In this paper we have dealt with data collected under conditions which posed only a *hypothetical threat* to the target populations and no arbitrary restrictive conditions were in effect. An adequate response was generated, however, for the purpose of analyzing some of the mechanics of social diffusion.

We have concentrated on an analysis of *age* as an influence in social diffusion. That is, our attention has been focused on the comparative role of adults and children in the learning of a message, the retention of its content, and the compliance with the leaflet's intent.

The study of these relationships may have considerable practical significance for propagandists. While we emphasize that the findings reported here are relative to the specified test conditions, we also suggest that they may have a practical potential beyond these restrictions. This is so because there is a high probability that the communications role of children is quite consistent under several geographical, ideological, psychological, and social conditions. The restrictions in movement and social intercourse that are placed in adults, either through normal commitments or arbitrary fiat, are not always applicable or enforceable with children even under the severe conditions of totalitarianism. Thus it is plausible to hypothesize that the active and vigorous communications role displayed by children under the test conditions discussed would be found under many other conditions. This hypothesis would be worthy of further investigation.

While we have particularly noted the active role of children in social diffusion and their keen ability in message recall, we must not overlook the possibility that adults fulfill a qualitatively different function in the diffusion process. Thus while children are undoubtedly important *neutral transmitters* of the message, adults, by virtue of their developed status position in the community and their advanced verbal and comprehensive skill, not only transmit the message, but may also influence its reception, directly or indirectly, negatively or positively.

The effectiveness of any form of communication is commonly thought to depend to a considerable extent upon *who* delivers it. Thus the impact and the credibility of a message depends in part on the general character of the source, or, more precisely, depends on how the audience perceives the general character of the communicator and whether they recognize his role as involving *origination* of the message, *endorsement* of the message, or mere *transmission* of the message.

If, for example, a prestige source in the community (a person with high status) endorses a message while passing it on to others (even if this endorsement is only the mere act of his bothering to pass it on), it may be hypothesized that it will have a greater impact than if it were merely received directly from the medium or casually accepted from some active youngster. In this particular study we do not have any direct evidence on message impact resulting from the age status of the social diffusion source. However, we have indicated the extent to which adults engage in the social diffusion of a leaflet message, and we have noted that adult-instigated interaction is mainly directed toward other adults and not toward children. Subsequent studies on social diffusion of propaganda messages might profitably be directed toward examining the following questions: (1) To what extent is the message-passer perceived as the originating source of the message, as endorser of the message, or as a mere neutral transmitter of the message? (2) How does the retention, acceptance, and compliance with the message vary with the perceived status and credibility of the passer?

We suspect that these are crucial issues in determining the impact of the propaganda content, particularly when social diffusion involves adult-adult or child-adult relations. In this latter instance, contact is readily achieved largely because of the intiative and high mobility of children. However, there is little evidence to suggest any critical effect on communication (the understanding of the message) when extreme age groups interact. These propositions should, in any case, be subjected to definitive empirical test.

OFFICIAL REPORTS AND PROCEEDINGS

NOTICE CONCERNING THE 1955 ANNUAL MEETING

The fiftieth annual meeting of the Society will be held at the Shoreham Hotel, Washington, D. C., August 31 – September 2, 1955. President Donald Young and the 1955 Program Committee have arranged the following sessions and chairmen:

Racial and ethnic intergroup relations
George Simpson, Oberlin College

The family
Nelson N. Foote, University of Chicago

Industrial sociology
Wilbert Moore, Princeton University

Crime and penology
Lloyd E. Ohlin, University of Chicago

Population
Conrad Taeuber, Bureau of the Census

Social stratification
Leonard Broom, University of California

Social psychology
Edgar F. Borgatta, Harvard University

The community
Roland L. Warren, Alfred University

Religion
Samuel Blizzard, Pennsylvania State College and Union Theological Seminary

Political sociology
Hans Speier, Rand Corporation

The measurement of public opinion
John Riley, Rutgers University

Problems of aging
Henry Sheldon, Bureau of the Census

Sociology and the professions
William J. Goode, Columbia University

Sociology and mental health
Edmund H. Volkart, Stanford University

Sociology and health
Lyle Saunders, University of Colorado

Sociological theory
Robert Bierstedt, College of the City of New York

Recent mathematical and statistical development
Daniel Price, University of North Carolina

Sociology and social welfare
Ernest Greenwood, University of California

Sociology and education
Neal Gross, Harvard University

Labor force
Evelyn M. Kitagawa, University of Chicago

Members may submit papers directly to chairmen, or, if there is any doubt as to the appropriate chairman, to the Program Committee, in care of Conrad Taeuber, Bureau of the Census, Washington, 25, D. C. Papers should not exceed 1500 words in length, and must be received by February 1, 1955, at the latest.

In addition to these sessions, arrangements will be made for a number of special activities (a tour of the Census, for example) and for the demonstration of statistical machine operations under the direction of Robert Bower. It is anticipated that joint programs will be arranged with the Rural Sociological Society and the Society for the Study of Social Problems.

NEWS AND ANNOUNCEMENTS

☆

United Nations Congress on the Prevention of Crime and the Treatment of Offenders. The first Congress will be held at the Palais des Nations, Geneva, Switzerland, from August 22 to September 3, 1955.

The agenda will include the following items:

(1) standard minimum rules for the treatment of prisoners;
(2) selection and training of personnel;
(3) open institutions;
(4) prison labor;
(5) juvenile delinquency.

Midwest Sociological Society. The new officers of the Society are John Burma, President; Marston McCluggage, Vice-President; Arnold Rose, Research Committee; and Stuart Queen, Representative to the American Sociological Society. The 1955 meeting will be held in Des Moines, Iowa.

The Social Science Research Council is sponsoring a summer inter-university research seminar at Brandeis University, on problems of bridging the gap between field studies and laboratory experiments on social behavior. Seven psychologists and sociologists from four universities will assist each other in planning research designed to test findings from laboratory research in natural situations, or vice versa. The participants are: Henry W. Riecken, Chairman, and Leon Festinger, both from the University of Minnesota; Nicholas J. Demerath and John W. Thibaut, University of North Carolina; Morton Deutsch and Richard Christie, New York University; and Gilbert Krulee, Tufts College. The approximate dates of the seminar were June 28 through August 7.

The Social Science Research Council has received a Lord and Taylor Award in the sum of one thousand dollars, for increasing knowledge of man and human relations through its advancement of research and training in the social sciences. The award was presented at a luncheon in the grand ballroom of the Waldorf-Astoria on Monday, May 3.

Air Force Personnel and Training Research Center. Rupert B. Vance, University of North Carolina, spent the month of June with the Laboratory at Maxwell Air Force Base, Alabama, as consultant to the Manpower Research Division on problems of the Air Force officer population.

C. A. McMahan, formerly of the University of Georgia, is Chief of the Manpower Research Division. Other sociologists in the Division include Thomas R. Ford, formerly of the University of Alabama, Jerry Walker Combs, Jr., formerly of Emory University, Charles B. Nam, formerly with the Bureau of the Census, and Stephen W. Fotis.

Boston University. The Department of Sociology and Anthropology of Boston University has made the following appointments to its staff: William O. Brown, formerly Chief, African Branch, Division of Research for the Near East and Africa, United States Department of State, as Professor of Sociology in charge of the newly established Graduate Program of African Studies and Research; George Horner, to the staff of the African Studies and Research program; and Daniel F. McCall, who is completing his dissertation for the Ph.D. degree in sociology at Columbia University, as Instructor in Anthropology. Mr. McCall has recently returned from three years of teaching and research in Africa and will devote part of his time to research in the African Studies Program and part to teaching.

Alvin D. Zalinger, Instructor in Sociology, will be on leave of absence during the academic year 1954–1955 to carry out research studies of African Students in the United States, supported in part by a grant from the Twentieth Century Fund, and also under the auspices of Boston University's Graduate Program of African Studies and Research.

T. Scott Miyakawa resumed his teaching in the fall, following a year's leave of absence as a Fulbright appointee at Doshisha University, Kyoto, Japan.

C. Wesley Topping, of the University of British Columbia, taught in the 1954 Summer Term of Boston University in exchange with Albert Morris, Chairman of the Department of Sociology and Anthropology, who taught in the Summer Session at the University of British Columbia.

Brooklyn College. The Department of Sociology and Anthropology joined the Department of Education in conducting a Workshop in Intergroup relations July 1 to August 3, 1954. The National Conference of Christians and Jews and the Anti-Defamation League of B'nai B'rith cooperated with the College in this program. Feliks Gross of Sociology and Helen Brell of Education directed the workshop, aided by a group of nationally known experts in intergroup relations. Three hours of graduate credit in sociology and three in education were earned in the workshop.

Herbert H. Stroup has been appointed Dean of Students in the College of Liberal Arts and Science. He has also been promoted to Professor of Sociology and Anthropology.

LeRoy Bowman was promoted to the rank of Associate Professor on February 1, 1954.

George Simpson, Assistant Professor of Sociology and Anthropology, is serving as Chairman of the Interdepartmental Graduate Committee on Social Studies.

S. M. Miller, Assistant Professor in Sociology and Anthropology, was appointed Chairman of the interdepartmental Social Science Group in September, 1953. Professor Miller had served as Executive Secretary for this Group since its inception five years ago.

Hugh H. Smythe, who recently returned from Japan where he and his wife, Dr. Mabel M. Smythe, held posts as Visiting Professors at Yagamuchi National University, is now giving a full program in the department.

Herbert A. Bloch of St. Lawrence University has been appointed Associate Professor of Sociology and Anthropology.

Emory University. In the fall of 1954 the Department of Sociology will offer a newly-designed course of study leading to the Master of Arts degree. Enrollment is limited to one student per staff member. Applicants may qualify for fellowship and assistantship awards totaling up to $2,125.00 for the first year of graduate study.

Helmut Schoeck, formerly Visiting Research Fellow in Sociology at Yale University, joined the department in the fall as Assistant Professor of Sociology.

Dudley E. De Groot joined the department in January from Ohio State University, and has accepted a regular appointment as Instructor in Sociology for the coming year.

George Williams College. Sylvanus M. Duvall has been granted a leave of absence so he may accept an invitation from the World Council of Churches to work with his wife, Evelyn M. Duvall, in jointly conducting conferences on family relations for church leaders, especially in Asia. The conferences will take the Duvalls to Japan, Manila, India, Palestine, and various countries in Europe.

Harvard University. Carle C. Zimmerman was appointed Visiting Research Professor at the University of Rome for the nine months following June, 1954. The research involves a cooperative study of Italians in Europe and in the United States, using the techniques of sociology, anthropology, and the medical sciences. In addition, a short study of certain Italian villages may be made, using some of the techniques of Zimmerman's Siamese study in 1930–1931.

Indiana University. Joseph Schneider was engaged in research on the sociology of war during his sabbatical leave Spring semester.

Frank R. Westie was granted a summer research fellowship from Indiana University Graduate school for research on race relations.

Teaching positions have been accepted by the following doctoral candidates: John Reiling at DePauw, Charles Hobart at Redlands, Robert Bell at Temple, and Henry Hart at Western State College of Colorado. Robert Hardt has taken a position with the New York State Mental Health Commission.

University of Minnesota. Elio D. Monachesi, Chairman of the Department of Sociology, and Starke R. Hathaway, Professor and Director of Clinical Psychology, received a grant from the United States Public Health Service for a longitudinal study of social adjustment. The grant was made available January 1, 1954.

Lowry Nelson has received a Fulbright research grant and sabbatical leave for 1954–1955. He will study village social organization in Italy.

George Vold has been on a sabbatical leave studying penal systems in several European countries.

Arnold Rose was appointed consultant to the Human Resources Research office of the United States Army.

Theodore Caplow has received a Fulbright grant and sabbatical leave for 1954–1955. He will lecture at the University of Utrecht in Holland.

Leo G. Reeder was awarded a grant from the Graduate School for a study of "Social Factors in Cardiovascular Diseases." The research is being done in the Laboratory of Physiological Hygiene of the School of Public Health. He was also appointed a consultant to the Minnesota Heart Association and is directing a study of attitudes and practices of employers toward cardiac workers. George A. Donohue is field supervisor of this project.

Mississippi State College for Women. Evelyn Ellis, Associate Professor of Social Studies, resigned her position and was married this summer to Glaister A. Elmer, Assistant Professor of Social Science at Michigan State College.

University of New Mexico. Helen Ellis has returned to staff duty in the social work curriculum after a year's sabbatical leave spent at Smith College and Boston Psychopathic hospital.

A summer workshop in intercultural relations was conducted under the joint sponsorship of the department and the College of Education at the University. The workshop was under the directorship of Stewart Cole of the National Conference of Christians and Jews.

The department is sponsoring an Institute of Labor Relations in cooperation with the New Mexico State Federation of Labor. Ellis Scott is director of the Institute.

University of North Carolina. E. William Noland was appointed Chairman of the Department of Sociology and Anthropology for the five-year period beginning July 1, 1954.

Howard W. Odum and Lee M. Brooks relinquished the co-chairmanship of the department in the summer, 1954. Odum retired in September, 1954, to devote himself entirely to writing, under

provisions of a three-year Guggenheim grant. Brooks will devote his entire time to teaching and writing.

Harold D. Meyer was visiting sociologist at Winthrop College, Rock Hill, South Carolina, in March. Between April 4 and 7, Professor Meyer directed a second Southern Regional Conference on Recreation for the Aging at Chapel Hill.

Wallace E. Lambert, of the Institute for Research in Social Science, attended a work conference on psychology and linguistics at Columbia University in May sponsored by the Social Science Research Council.

John Thibaut and N. J. Demerath of the Institute for Research in Social Science took part in a six-weeks invitational seminar at Brandeis University near Boston beginning in late June.

Rupert B. Vance is one of six social scientists appointed to the Sociology Panel for Anthropological and Related Sciences of the National Research Foundation. During June he served as Consultant to the Manpower Research Division of the United States Air Force at Maxwell Field, Alabama.

John J. Honigmann continued his study of Pakistani culture this last summer at the University of Pennsylvania, on a grant from a special fund for the Institutional Exchange of Personnel provided by the Ford Foundation and administered by the University of North Carolina's Institute for Research in Social Science.

Reuben Hill returned in the fall after his leave of absence for one year in Puerto Rico where he directed field work on "Family Size and Fertility Planning," a project sponsored jointly by the United States Public Health Service and the Institute for Research in Social Science at the University of Puerto Rico. Professor Hill and Visiting Professor Leslie, as directors, assisted in the planning and direction of the seventeenth Groves Conference in Marriage and the Family held April 28–30 at Purdue University. In recognition of the twentieth anniversary of the first Groves Conference held in Chapel Hill in 1934, special tribute was paid to the pioneering leadership of Ernest R. and Gladys H. Groves.

Nicholas J. Demerath of the Department of Sociology and Anthropology has been elected a vice president of the Society for Applied Anthropology.

Cheng K'un Cheng of the University of Hawaii, and William E. Cole of the University of Tennessee, were Visiting Professors in the Summer Session, 1954.

Gerald R. Leslie, Visiting Professor for 1953–1954, returned to Purdue University after teaching in the first summer session.

Northwestern University. Thomas D. Eliot has become Professor Emeritus of Sociology at Northwestern after thirty-five years of service there. During the Spring semester he was Visiting Lecturer at the University of Wisconsin.

University of Puerto Rico. The Social Science Research Center of the University of Puerto Rico has had four major projects in operation this past year, all of which continued during the year beginning July, 1954. Most of the financial support of the research is derived from the University of Puerto Rico and is supplemented by grants from foundations and public agencies.

The Family Life project is directed by Reuben Hill, on leave from the University of North Carolina, and J. Mayone Stycos in collaboration with Kurt W. Back, research associate. The project is in its third year of operation and is being financed this year on a matching basis with the University by The Population Council, Inc., of New York. Its focus this year is upon quantitative verification of hypotheses about the consequences for human fertility of family patterns and interpersonal relations within the family. The final phase of the research is to be conducted collaboratively with Guillermo Arbona and Robert R. King, Jr., of the Department of Preventive Medicine and Public Health of the University's School of Medicine.

The Social Stratification and Mobility Project in Puerto Rico is being directed by Melvin Tumin, on leave from Princeton University, with the assistance of Arnold Feldman, Ph.D. Candidate in Sociology at Northwestern University. Carlos Albizu Miranda, Clinical Psychologist at the University of Puerto Rico, is Research Associate in charge of psychological testing.

In addition to the basic financing provided by the University, supplementary funds have been secured from The Ford Foundation and the National Institute of Health. Under the latter's subsidy, a special study of Personality and Culture in Puerto Rico is being conducted.

The study consists of an investigation into the different kinds and levels of readiness of various segments of the population of Puerto Rico for alternative possibilities in the Island's future, especially those now visible in the ongoing industrialization, urbanization, and secularization of the Island. Accordingly, an area probability sample of one thousand persons has been interviewed with regard to objective statuses, life chances, institutional patterns, attitudes, and personality structure. The sample is stratified into five educational groups and three urban-rural residential groups.

The Manpower Resources Project is under the direction of Lloyd G. Reynolds with Peter Gregory and Luz Torreullas serving as Assistant Directors. The project is undertaking a broad survey of manpower problems associated with the accelerated industrialization of Puerto Rico.

The Puerto Rico-United States Economic Relations Project is directed by Walter Isard of the Massachusetts Institute of Technology. Its purpose is to identify new markets for Puerto Rican industrial products in the New York and Gulf Coast areas. Its method is to forecast the economic expansion of these regions, list and quantify the inputs necessary for this expansion,

and by technological and cost accounting analysis to determine which of these inputs could be produced in Puerto Rico at a cost advantage permitting successful competition with other suppliers.

Purdue University. Louis Schneider has been promoted to full Professor. In addition, he has been granted a leave of absence for the 1954–1955 academic year and has accepted a fellowship appointment by the Division of Behavioral Sciences of the Ford Foundation to participate in the new Center for Advanced Study during its first year of operation.

Gerald R. Leslie has returned to resume his duties at Purdue with the starting of the Fall semester. During the past year he was on leave of absence at the University of North Carolina.

A new staff position has been created in urban-industrial sociology and Robert L. Eichhorn has been employed to fill it. Dr. Eichhorn recently completed his degree at Cornell University, where he studied in the Department of Sociology and Anthropology and the School of Industrial and Labor Relations.

Vanderbilt University. At the request of the Vanderbilt Committee of Educational Inquiry of the Carnegie Foundation, the Department of Sociology and Anthropology is conducting a study of changes in student's values. The research is based on a revision of the 1950 version of the Cornell Values Study questionnaire and is under the direction of Jay Artis.

Emilio Willems spent the summer in Portugal doing research on the structure of the Portuguese family in relation to the societal class structure. This study is subsidized by grants from Vanderbilt University and the Social Science Research Council.

University of Washington. Clarence Schrag has been granted a leave of absence to serve as Supervisor of Adult Correctional Institutions for the State of Washington. He will return to the department in 1955 or 1956.

S. Frank Miyamoto was awarded a grant for summer research by the Graduate School, in order to plan a project on small group studies. He has also had a part in forming an organization of psychologists and sociologists who are interested in research in the field. Sociologists on the west coast who are interested in joining the group may receive information from Dr. Allen Katcher, Department of Psychology, University of Washington.

Sanford M. Dornbusch has been granted a leave of absence to work at the Center for Advanced Study in the Behavioral Sciences, in Palo Alto, California. He will pursue inquiries in assimilation and role-taking.

Glen T. Nygreen, who was awarded the Ph.D. degree in June, 1954, has a position as Dean of Men and Assistant Professor of Sociology, at Kent State University, in Ohio.

William R. Catton, Jr. has been appointed to an instructorship at Reed College, for the year 1954–1955.

Aubrey Wendling, who is near the point of completion of the Ph.D at this University, and John Kitsuse, at about the same stage of training at U.C.L.A., have been appointed to teach the courses of Schrag and Dornbusch for the present academic year.

Nahum Z. Medalia, who has completed the final report on the Air Site Project, has accepted the position of Assistant Professor in Social Sciences at Georgia Institute of Technology.

* * *

The *Review* regrets to report the deaths of three of its members:

Walter Richard Harrison, who died in January, 1954, had been a member of the Society since 1946. His undergraduate training, in Economics and Sociology, was at Howard University, where he received the B.S. in 1930. He earned an M.S. in Economics, and a Ph.D. in Sociology at Cornell University, receiving the latter degree in 1944. He taught for seventeen years and was engaged in the practice of social work in Detroit and Philadelphia. He was the author of articles on attitudes toward churches.

Albert B. Hines, who died September 26, 1953, had been Executive Director of Madison Square Boys' Club in New York. He was trained at Columbia University, and became an Associate Member of the Society in 1950.

Herbert D. Lamson, Professor of Sociology at Boston University, died March 12, 1954. He received the Ph.D. degree at Harvard in 1935, and taught at the University of Shanghai, and the University of Maine. He was the author of *Social Pathology in China,* and several articles.

BOOK REVIEWS

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The Social Psychology of Prejudice. By GERHART SAENGER. New York: Harper and Brothers, 1953. xv, 304 pp. $400

The Nature of Prejudice. By GORDON W. ALLPORT. Cambridge: Addison-Wesley Publishing Company, Inc., 1954. xxiii, 537 pp. $5.50.

These two books, published almost simultaneously, represent a new development in the study of relations between racial, religious, and ethnic groups. While psychologists have engaged in specific studies of intergroup prejudice at least as far back as the 1920's, they have hitherto left to sociologists the writing of systematic treatises on intergroup relations. If these books herald the sustained interest of psychologists in the field, one can call attention to certain changes of orientation that will take place: (1) Attention will be shifted from the *behaviors* involved in intergroup relations, especially those of discrimination and conflict, to the "psychological set," or prejudice, with which the members of groups view each other. Saenger presents only a summary, background chapter on objective behavior, and Allport merely refers his readers to other sources. (2) Attention will be shifted from historical and cultural analysis of the group to psychological analysis of the individual members of the group. Both Saenger and Allport acknowledge the important role of "culture," but devote the bulk of their space to such topics as the structure and etioligy of the prejudiced personality, "scape-goating," the perception of group differences, etc. Both authors conscientiously deal with economic, political, and social variables, but their primary concern is with the effects of these on the individual rather than with the historical setting in which these variables are integrated in certain ways into the structure of the society. These psychological emphases will be healthy and stimulating approaches to the study of an important subject matter if they do not result in the neglect of the sociological emphases.

Both books are products of good workmanship. The authors have covered most of the recent literature on the origins and nature of prejudice and have organized it in logical and systematic form. Saenger especially has produced a highly readable volume. Its orientation is toward the practical—its subtitle is "Achieving Intercultural Understanding and Cooperation in a Democracy" and Saenger does a thorough job of presenting all of the known techniques of combatting prejudice. Though scholarly and well-balanced, he has produced an invaluable handbook "for all those who in the pursuit of their work have to deal with the prevention and cure of prejudice and discrimination."

The book starts out with a statement of the scope of the problem but immediately goes into the practical question of "the cost of prejudice." The facts about actual group differences are then summarized, not so much to analyze group differences as such, but to provide the armament for attacking false stereotypes. In dealing with the causes of prejudice, Saenger takes the realistically eclectic position that a number of economic, political, sociological, and psychological variables must be considered. In presenting techniques for reducing prejudice, Saenger again avoids the temptation to press for any one approach, but offers a balanced strategy and outline of tactics that includes the use of law as well as education, propaganda and social contact. There is practically nothing that is new in this volume, but it is a praiseworthy summary of relevant knowledge presented in such a form that it should have maximum usefulness for those who seek to effectuate social change. Perhaps a minor defect is the impression it unintentionally conveys that we know more than we actually do.

The Allport volume is more ambitious in the sense that it seeks to lay a basis for future thought and research on prejudice as well as to present a summary of existing knowledge. While it has two chapters on techniques for reducing prejudice, its main purpose is exposition of the nature of prejudice. Its scope is therefore broader than that of the Saenger volume. Allport starts out with a philosophical analysis of the concept of "prejudice" and seeks to relate it to basic elements in human nature. He quickly moves to the sociological processes involved in the formation of in-groups and the rejection of out-groups. Four chapters are devoted to group differences, varying from the purely physical ones to the intangible (and unsubstantiated) "traits" due to victimization. A consideration

of how these group differences are perceived and integrated into general thought processes takes the form of a discussion of the cognitive process, the nature of language, stereotypes in our culture, and factors in prejudice. The last-mentioned topic is then broken down into fine subcategories and dealt with at great length: examined are culture patterns, societal structure, patterns of social contact, the processes of learning, ideological conflict, frustration, aggression and hatred, anxiety, projection, the structure of prejudiced and tolerant personalities and the impact of propaganda on them. The book closes with a brief consideration of the techniques of reducing prejudice and their evaluation. There are brief discussions of methodological considerations which arise in the study of prejudice.

Allport's approach, like Saenger's, is eclectic, and therefore it is not possible to characterize the book as having a distinctive point of view. It does have a certain peculiar quality, which the author reveals unintentionally. While I find it difficult to characterize this quality, I can convey something of its flavor by labelling it "naturalistic." Prejudice is presented as a natural phenomenon, as an inevitable manifestation of human nature. The characteristics of minority groups that members of other groups find objectionable, while distorted and stereotyped, nevertheless have a basis in fact. The manifestations of this orientation are varied. At one point, Allport bluntly states that "man has a propensity to prejudice" (p. 27) without showing the relation between his use of the term prejudice and the phenomena of racial and ethnic antagonism that he considers in the bulk of the book. He refers to the "bigot and nonbigot" as equally biased on race matters (p. 85), which suggests that the true scientist should take a position somewhere between these two reprehensible creatures. A different manifestation of this "naturalism" is the abandonment of the terminology of analyzing group differences as physical, mental, and cultural for a methodological classification that includes "a rare-zero differential" and "a categorical differential." In discussing the overlap of traits between racial groups, Allport goes to the irrelevant extreme of referring to albino Negroes, when Negroes who can readily pass as whites would serve the purpose better (p. 103). He approvingly quotes Talcott Parsons: "If Jews could be evenly distributed through the social structure, anit-Semitism would probably be greatly reduced," without indicating that the sentence is at least equally accurate if the proposition were inverted to: "If anti-Semitism would be greatly reduced, Jews could be evenly distributed through the social structure."

In a book as long and on such a difficult subject as Allport's, mistakes are bound to occur. For example, there is obvious need to describe and explain group characteristics, and yet this is likely to lead to overgeneralization. While trying to be cautious, Allport does say that Americans are more punctual than Western Europeans (p. 97), "to an outsider a Lutheran is a Lutheran" regardless of what synod he belongs to (p. 134), "the imposing synagogues in Jewish residential districts make for visibility" (p. 249). Freud's theory of anti-Semitism, while treated with greater approval than it perhaps deserves, is distorted: according to Freud, the Jews are hated, not because they killed Jesus (which is what Allport thinks Freud says) but because they killed Moses and refuse to expiate their guilt through accepting the penitence of Jesus. While making excellent use of case material brought in by Harvard students, Allport sometimes fails to correct for bias created by this source: for example, he states that rumors derogatory to Negroes are less numerous than rumors derogatory to Jews (p. 65) without indicating that his information was from the Boston area. Also the Northerner's lack of knowledge of Southern race relations is indicated by his interpretation of most Southerners' claim, that Negroes are satisfied, as "a tribute to the Negro's protective concealment behind a mask" (p. 147). Negroes do have some protective concealment, but so do white southerners.

An underlying difficulty, afflicting both Allport and Saenger, is their apparent failure to realize that American *culture* is partly racist. This racism is accepted unthinkingly and provides a framework for perception, thought, and action. It has varying forms and degrees in different sections of the country, and is now declining generally. The psychological, and even the "social" variables, that these authors emphasize in their analysis of the causation of prejudice are very powerful, but nevertheless they influence American behavior specifically in an anti-minority direction because of the definitions which racism provides for them to work on. This defect, while a crucial one, does not invalidate the carefully worked-out theories and supported conclusion of Allport; it merely leaves a logical gap to mar an otherwise superior analysis. Both authors are probably open to correction since they strive so hard to develop a properly balanced interpretation. Allport especially is creative when he is reconciling psychological and sociological influences, as in his suggestion that psychological *identification* may play an important role in

the learning of culture (p. 293). These authors set for themselves a difficult task and have come through remarkably well.

ARNOLD M. ROSE

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Studies in the Scope and Method of "The Authoritarian Personality:" Continuities in Social Research. Edited by RICHARD CHRISTIE and MARIE JAHODA. Glencoe, Illinois: The Free Press, 1954. 279 pp. $4.50.

This is the second volume in the series, *Continuities in Social Research.* Like the first volume on *The American Soldier,* the present one on *The Authoritarian Personality* consists of a series of essays which, through criticism and positive contribution, perpetuate the line of endeavor represented by the parent work.

Edward Shils, in his essay on "Authoritarianism: 'Right' and 'Left,'" makes explicit the more serious consequences of the Berkeley group's tacit endorsement of the obsolete belief that all political, social and and economic philosophies can be classified on the "Right-Left" continum. Among these consequences the following are perhaps most significant: for the most part only the fascistic, "authoritarian of the 'Right,'" is studied directly; they define their "complete democrat" residually as the direct opposite of the "fascist." Thus they blur "the distinction between Leftist authoritarians and reasonable persons of humanitarian, equalitarian dispositions." They not only fail to study the Communistic "authoritarian of the Left"; they fail to recognize his existence.

Harold Lasswell, in his article "The Selective Effect of Personality on Political Participation," presents a theory of the development of the "power-centered personality." This theory is a unique contribution in that it combines the approach of psychoanalysis with that of the political scientist.

In their "Methodological Critique" Herbert Hyman and Paul Sheatsley conclude that ". . . the authors' theory has not been proved by the data they cite . . . ." If one follows their painstaking analysis from start to finish, he cannot help but agree with their conclusion. Richard Christie, in "Authoritarianism Re-examined," is equally rigorous in his criticisms of the parent volume. He also examines the myriad subsequent researches which have taken their cue from *The Authoritarian Personality.* These appraisals by Hyman and Sheatsley and by Christie are models of methodological criticism and would in themselves justify the use of this book as a companion-volume in a course on research methods.

The long-range investigation of ethnocentrism in children reported by Else Frenkel-Brunswik in "Further Explorations by a Contributor to 'The Authoritarian Personality'" is an important addition to the understandings provided by *The Authoritarian Personality.* From the standpoint of approach this project is superior to most of the investigations reported in the original work: there is somewhat greater concern with the societal and cultural contexts within which ethnocentrism and its psychological correlates develop.

A substantial portion of Frenkel-Brunswik's essay is, in effect, a rejoinder to numerous criticisms which have been leveled, both in this book and elsewhere, at the original volume. Of her many qualifying remarks, we have selected the following for comment here because it is a qualification (and also a revelation) of one of the most important shortcomings of *The Authoritarian Personality*—a shortcoming which is not given adequate consideration in the book under review: "Choice of the approach-via-personality is especially called for when, as is true in our case, objective social structure is taken for granted and constitutes no more than the common background of an investigation centered about finding individual differences in the appeal various elements of a common social background may exert upon varying personalities. It must be remembered that it has never been our aim to generalize from our individuals to the culture as a whole; since we described a variety of fairly distinct personality syndromes existing in one and the same culture, such inference could not even have been justifiably attempted. If our major interest would have been to assess the predilection toward authoritarianism in different societies we would have had to concentrate on a comparative study of the institutions of these societies, and this we did not set out to do."

The above position, however, is nowhere clearly stated in the parent book, nor is this recognition of cultural phenomena incorporated in any meaningful way into the theory or conclusions of *The Authoritarian Personality.* Moreover, this qualification as it stands is not quite adequate. It cannot be assumed that the people studied share a common cultural background. If two people differ in their images of and hostility toward the members of a particular outgroup, these differences cannot be attributed primarily to personality characteristics peculiar to one or the other of these individuals *unless the investigator has considerable knowledge regarding the normative system provided by each individual's family, school,*

social class, local community, "region," etc. Cultural (including *sub-cultural*) norms defining relationships with particular out-groups in harshly negative terms often persist long after the functional necessities (psychological or social or both) which in past generations fathered these hostile norms have ceased to exist. It is perhaps for this reason more than any other that the "happy bigot" is so much more common than the psychoanalytic approach would lead us to believe. The relative availability of prejudicial and non-prejudicial norms is a datum which must be considered in any "causal" analysis of prejudice on the individual level.

Despite the incisiveness of its criticisms, this book as a whole is likely to leave one with a feeling of greater respect, not less, for the parent work. This is not only due to the substantive contributions of Lasswell and Frenkel-Brunswik; it is also due to the fact that the conspicuously negative critics, Shils, Hyman and Sheatsley, and Christie are rarely disdainful, rarely out to demonstrate their own erudition, but rather engaged in a meticulous, sometimes laborious task of criticizing a very significant work in order that it may be improved.

FRANK R. WESTIE

Indiana University

Facial Deformities and Plastic Surgery: A Psychosocial Study. By FRANCES COOKE, MACGREGOR, THEODORA M. ABEL, ALBERT BRYT, EDITH LAUER, and SERENA WEISSMANN. Springfield, Illinois: Charles C Thomas, 1953. xv, 230 pp. $5.75.

An interdisciplinary group consisting of "the plastic surgeon, the sociologist-coordinator, a home investigator with anthropological training, a psychiatrist, and two clinical psychologists" reports in this volume on a pilot study of the relation of facial deformity "to personality structure and social adjustment, as well as the effects of plastic surgery on this adjustment." The 74 patients, studied by interviews and projective tests, vary considerably in type of facial deformity, age, socio-economic and ethnic backgrounds. Twenty-seven had completed plastic surgery, while 28 were in process of surgery when the study ended.

Four detailed case histories illustrate the type of data obtained in interviews. The materials are lively, human, and interesting. The authors succeed in giving the reader a feeling for the patients as persons in continuing interaction with others. Each of the authors has written a separate chapter, interpreting the case material from his own point of view, and drawing attention to: (1) the values society places on deformity, (2) the way in which these are transmitted to patients by parents and others, (3) the role played by deformity in personality structure, and (4) the results of surgery in terms of adjustment.

The chapter on "psychosocial aspects," by MacGregor, is likely to be of greatest interest to sociologists. The deformed person is depicted in interaction with others, interpreting and evaluating the reflections of self which come back from them. ". . . Socially created images held of the deformed often become distorting mirrors which in turn reflect their personalities and which can be significant factors in controlling their behavior and social adjustment" (p. 81). Three types of adjustment are suggested to cover the range of cases: (1) withdrawal or avoidance of others, (2) overt aggression, and (3) attempts to do something about the deformity, such as excessive charm, joking remarks, and the like. Having adopted this approach to the data, it is strange that the author fails to mention C. H. Cooley, G. H. Mead, or W. I. Thomas, although *Mind, Self, and Society* is listed in the bibliography.

Lauer's chapter on the family reports data obtained by visiting the families of patients in their own homes. Students of the family who rely mainly on data obtained outside the home should take warning from her finding that "sometimes the mother's description of the relationships within the family did not jibe with the home investigator's observations" (p. 104). The chapter on "psychological aspects," in which Abel and Weissmann interpret the cases with the help of projective tests, will be mainly of interest to clinicians accustomed to the manipulation of this type of data. The conclusion of Bryt's psychiatric chapter with respect to the importance of plastic surgery in assisting adjustment is consistent with findings elsewhere in the book: some cases were assisted, in others personality seemed relatively unchanged, while in still others surgery resulted in new problems or redefinition of old ones. The concluding chapter consists mainly of practical recommendations intended to be useful to those who deal professionally with persons who are facially deformed.

In general the book leaves something to be desired as a research document. The preservation of individuality in the inter-disciplinary approach is an advantage in that it presents many facets of the problem, although with some repetition, but a disadvantage in that it makes for a lack of integration in the conceptualization of data. One is entitled to express the pious hope that the pilot study will be followed by one characterized by a more inte-

grated conceptual framework, which will attempt to bring together the various interesting and insightful typologies scattered through the present chapters.

Readers who are mainly interested in fine points of research design and sampling will be unimpressed. Others, like this reviewer, who consider it worthwhile to report impressionistic findings based on the exploration of new areas, will find the book rewarding. In particular, the chapter of case histories, and that on "psychosocial" aspects will be stimulating to followers of a symbolic interactionist approach to social psychology.

DAVID N. SOLOMON
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Interrelations Between the Social Environment and Psychiatric Disorders. Edited by FRANK G. BOUDREAU and JEAN DOWNES. New York: Milbank Memorial Fund, 1953. 263 pp. $1.50.

This is a collection of papers in the area of social psychiatry presented at the 29th (1952) annual conference of the Milbank Memorial Fund. There are six dealing with what has been called "Interrelations Between the Social Environment and Psychiatric Disorders," one each by representatives from public health, sociology, psychology, social psychology, biology, and anthropology. There are nine papers dealing with empirical research in this field in progress, completed, or proposed, and there is a report of a kind of panel discussion dealing with the "Definition of a Case for Purposes of Research in Social Psychiatry." All papers are comparatively brief. A part of the informal discussion which followed the presentation of each paper is included. The sessions were presided over by Alexander Leighton, who also contributed a report of the progress of the Stirling County study.

The sociological contribution to the first group of six is a creditable paper by Eugene Schneider dealing with some of the sociological concepts relevant to research in social psychiatry. The discussion of the relationship of role-content to personality disorientation (pp. 39–43) is well organized, and the paper includes a brief, sensible observation on the limitations of the analysis of small-scale social systems as a research method in this field. Sociologists will also find interesting the paper by Benjamin D. Paul which deals with the psychotic behavior of a Guatemalan girl analyzed in terms of what he has called the "self-regulating processes in culture."

The papers by the psychologist, William E. Henry, and the social psychologist, Marie Jahoda, point out the need for concern with what may be considered to be the positive aspects of social psychiatry, that is, concern with the identification of mental health as well as of mental illness. The paper by Leonard Scheele, Surgeon General for the Public Health Service, is a summary of the contribution which social psychiatry can make to the solutions of some of the problems of public health. The description by J. P. Scott of the socialization process among higher animals—dogs, for example—although it makes interesting reading seemed to this reviewer to be a little irrelevant to the theme of the symposium. The only criticism of these papers is in a certain looseness in the use of terms; in the fact that some terms which have distinctly different meanings were used interchangeably by the various participants and even, on occasion, by the same participant; and in the fact that some terms used, on occasion, by all were not defined by anyone.

With reference to the reports on research, the general orientation, if not the specific content, of some of these papers would be known to sociologists—the August Hollingshead and Frederick Redlich discussion of the social-class differences in the incidence of mental disorder, and the Joseph Eaton and Robert Weil further report on their research among the Hutterites, for example. There is a report of an ecological study by Ivan Belknap and Gartley Jaco which offers further evidence of neighborhood differences in incidence and form of mental disorder, but goes beyond earlier studies in suggesting that differences in distribution may be associated with the "type" of city studied in terms of economic base. A variety of other research projects are discussed in the remaining papers.

The general tone of this book is a lack of pretentiousness, a lack which is not to be confused, incidentally, with superficiality or with simplification for the benefit of a lay audience. None of the authors claims, or even suggests, that he is on the track of anything revolutionary, and some of the discussion—and some of the research described—are not particularly profound. This book represents a groping toward a real inter-disciplinary approach to the study of a very complex social problem. As such it merits the attention not only of sociologists but also of all interested in social welfare.

DONALD D. STEWART
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Science and Man's Behavior: The Contribution of Phylobiology (Including the complete text of: *The Neurosis of Man.*). By TRIGANT BURROW (Edited by WILLIAM E. GALT). New York: Philosophical Library, 1953. xii, 564 pp. $6.00.

This book consists of two major parts; one is based exclusively upon edited material which

Dr. Burrow gathered before his death as part of his plans for *Science and Man's Behavior,* and the second consists of an unchanged re-issue of *The Neurosis of Man,* published in 1950, that constitutes 404 pages of the present book.

According to the editorial foreword, a delay in publication of *The Neurosis of Man* provided Dr. Burrow in 1948 with the time necessary to present selected parts of it to eminent scholars in several relevant fields so that they could prepare considered appraisals. It is these which have been used for the first portion of the book. The statements of the twenty-nine authorities have been selected and organized around major problems suggested by chapter titles, two serving here as illustrations: "'Normality' and the Meaning of the Social Neurosis" and "Biological and Cultural Considerations." Among the twenty-nine authorities solicited were Herskovits, Kluckhohn, Krogman, Lindeman, Ogburn, and Sorokin. It seems clear that the purpose of including *The Neurosis of Man* is to put in hand for the thorough reader Dr. Burrow's conception of man, the theoretical system he has devised, and some experimental data in order to facilitate study of the first section.

The bibliography of articles and books of Trigant Burrow is more than respectably long. Upon completion of his training in medicine, he completed a doctorate in psychology at Johns Hopkins. His dissertation, published in 1909, could be classified as a study in psychophysics. After returning from studying with Jung, he was in practice and research in psychiatry and psychoanalysis until 1923 when he shifted to what he refers to as "social psychiatry" and "group method of analysis." In 1927 he became Scientific Director of the Lifwynn Foundation, and in 1928 he shifted his attention to research in phylopathology, a concern with "modifications of behavior induced through adjusting the organism's internal tensional patterns and a recording of the physiological changes accompanying these adjustments." His publications have been in medicine, experimental psychology, psychoanalysis, individual and social psychiatry, and phylopathology.

Since 1926 articles by Dr. Burrow have appeared in sociological journals. In 1927 *The Social Basis of Consciousness* was well received and appraised as a point of view holding considerable promise. Ten years later *The Biology of Human Conflict* was defined as a beginning fulfillment of that promise and continues in use by departments of psychology. But a review of it at that time by Anton J. Carlson was exceedingly terse, carried the overtone that this work is of little value to physiologists, and called attention to the extremely pessimistic point of view of the nature of man and his future. Others in 1937 still looked upon the approach as a promise.

Since Dr. Burrow has published in so many fields, it is clear that a specialist in only one field cannot appraise the range or significance of his work. He organized a group of investigators about him, set out to study tensions under varying conditions of attention, made use of the techniques of electroencephalography, and wrote books which appear to have a paucity of supporting data. But the more recent statements of Herrick and others raise questions that are more than just profound problems. The fundamental conception of man seems to be acceptable, but why all of the confusing categories and related concepts and "Lone Wolf" activity?

As one reviews the several books and reads *The Neurosis of Man,* one makes the preliminary estimate that Dr. Burrow had possibly made his most important contributions by 1937. His work, as he saw it and as he convinced a few others through inspiration, was a program of reforming our scientific approach to the study of man. To work out this program, he gathered a few followers, including his son-in-law who edited the present book, about him for the express purpose of setting up phylobiology. Apparently the world of scholars has paid little attention to this program and to phylobiology. It appears to be little known by social psychologists who have been trained in departments of sociology, and its findings and techniques have been subsumed and extended, so it seems, by social psychologists who have been trained in departments of psychology. *Science and Man's Behavior* may be the last effort in this program of reform. Most of us are likely to remain convincingly unreformed, for much of his work points to the existence of a central problem but offers no empirical proof that its solution has been advanced by phylobiology.

FORREST E. LAVIOLETTE
Tulane University

Three Men: An Experiment in the Biography of Emotion. By JEAN EVANS (Introduction by by GORDON W. ALLPORT). New York: Alfred A. Knopf, 1954. xviii, 297 pp. $3.75.

The reader would be well advised to begin this book early in the evening, because once its cover is opened he will be unable to lay it away until he reaches the last page. This is superb writing—the work of a craftsman in story telling. Its theme is the tragedy of emotional impasse.

Miss Evans is first of all a journalist and

writer. While doing feature stories for the now extinct newspaper *PM*, she developed what then appeared to the reviewer to be a new literary form, which combined the values of the short story, the newspaper article, and the case history of the psychiatric social worker. The development was a natural one for Miss Evans. Assigned to do the story of a crime and a criminal, she would allow her intelligence to seep itself in the facts of the case until she found herself involved in the problems of personality development and of social organization and structure. The difficulties presented *in concreto* by her empirical material led her gradually to look for what help she could get from the sciences of human behavior. Gordon Allport had the imagination to perceive the usefulness of her work for teaching in the academic disciplines. Despite the literary quality of these three case histories, they are destined to become the material for analyses in courses in sociology, social pathology, criminology, clinical psychology, psychiatry, mental hygiene, and social work.

Although her developing sophistication in the sciences of man is evident, Miss Evans has the good sense to allow the facts of her cases to speak for themselves. Certainly one perceives her interpretive mind at work in her assemblage of the facts. But there is a minimum of language deriving from one or another of the contemporary schools of human behavior. The consequence is that her material lends itself easily to the uses of the teacher in exemplifying the interpretive schemas of various schools and disciplines. The student will be moved by the material to raise many questions that should lead to fruitful discussion.

For the most part, the subjects of these biographies are allowed to tell their own stories, and these are supplemented by whatever records and information Miss Evans can obtain from institutions, doctors, and social workers. The core in the presentation of each case is the subject's own image of himself and of his relationship to others. There are movements in the direction of realistic self-appraisal (large in the case of Johnny Rocco and at a minimum in the case of Martin Beardson), and these are followed by speedy withdrawal into the closed circle of the original image. The reader can judge from the subject's own words, aided only a little by Miss Evans' impressions, the extent of delusion as well as the nature of the environmental factors that participate in the formation and the maintenance of the image of self. The interaction of this image with the changing realities of the environment make the drama of each case.

It has been said that Miss Evans' work will have the effect of challenging the social case worker to write in a more interesting manner. This may be true, but it does not do justice to Miss Evans. For what she has added to the case history is not merely the effect of craftsmanship in writing. Rather, she has shown how much can be disclosed by a passionate attachment to fact and an unhurried concern to understand the life of an individual. Certainly it would be utopian to expect the psychiatric social worker to have as much time and energy for each subject as Miss Evans had. But, at a time when sociology and the other behavioral sciences are engaged primarily in *extensive* research and in the development of large-scale interview techniques and statistical analysis, a fresh consideration of the fruitfulness of *intensive* research and its supplementary role can have only a wholesome effect. Such intensive research can be productive of ideas not only for the problems of psychodynamics but also for the problems of social structure and organization.

It may be useful to conclude this review with two instances of empirical insight in the book to illustrate this last point. Both are taken from the case of Johnny Rocco, the only individual among the three who shows signs of breaking through his emotional impasse. Johnny's early history was such as to make his case seem hopeless. He was one of eleven children, whose immigrant father died in a drunken brawl. Johnny was unloved by his mother, was the terror of his neighborhood, and he had a long record of juvenile arrests for robbery. Yet, he is now trying to be a good husband and father, and he is learning to read for the first time in his life. If Johnny now can maintain his heroic effort "to be good," the one factor in his environment most responsible (and this conclusion is not drawn explicitly by Miss Evans) was the assignment of O'Brien to his case. O'Brien was a counselor in an organization devoted to working with delinquent boys. Miss Evans' account of the therapeutic process developed by O'Brien throws the kind of light upon the value of such work that no amount of statistical follow-up studies could accomplish. The other empirical insight to be instanced here concerns the role of Johnny's anomic social enviornment in impeding Johnny's present efforts to recognize certain elementary norms of conduct as binding upon him. Johnny is deeply troubled, for example, by the normlessness of the representatives of the law. "And cops. Crooks! . . . They hang around in the barrooms . . . I see them take money off pimps and hustlers . . . I've been shook down myself." Of course, the concept of *anomie* is not a new

one in sociology. But it needs constantly to be enriched by *intensive* research. And, in the context of such material, discussions concerning the relations between "psychology" and "sociology" can become more meaningful to the student.

Incidentally, Miss Evans never uses, and I'm willing to bet has never heard, the word *anomie*. Sociologists can, nevertheless, hold converse with her.

JOSEPH NEYER

Rutgers University

Power of Words. By STUART CHASE (in collaboration with MARIAN TYLER CHASE). New York: Harcourt, Brace and Company, Inc., 1954. xii, 308 pp. $3.95.

Stuart Chase's *The Tyranny of Words* (1938) was one of the first works in the growing field of semantics read by the present reviewer. It left him with the impression that all "high order abstractions" are sinister at their worst, meaningless at their best. Further study of the question led this writer to revise the view and conclude that abstract terms are to be used with caution, but nevertheless used.

It is gratifying that Chase has come to a similar view. In his newer, more balanced work, he asks: "Why use abstractions at all, when they can be so dangerous to understanding? Why not always point to what you are talking about?" He answers himself with the further question, "Well, why use water at all when you can drown in it? Without abstractions we could not think in a human way. The problem is to be aware of them, to remember what level the discourse is on . . ." (p. 142).

Power of Words is divided into two parts. Part One presents—in the always readable style associated with Stuart Chase—some findings from a dozen different sciences which shed light on the role of language in human interaction. These include: brain physiology, cybernetics, psychology of perception, animal psychology, child psychology, cultural anthropology, linguistics, semantics, group dynamics, etc. The range of material is so large as to make the book seem *merely* eclectic. This weakness can perhaps be minimized if the reader can arrange to read the entire volume without any long interruptions.

Part Two, entitled "Applications," views such things as the writer's craft, economics, international propaganda, political campaigns, guilt by association, mass communications, etc., via the concepts which were introduced in Part One. The chapter on the mass media (and in conjunction with it the chapter on McCarthyism) will be of special interest to sociologists. Chase's suggestions for future research should be welcomed by the graduate student who is "looking for a thesis topic." The bibliography should be useful to sociologists at all levels.

The entire book, however, is germane to sociology. Various portions of it might profitably be assigned as "outside readings" both in survey and more advanced courses. The book is a useful introduction to what might be called the sociology of language. Language is not only the vehicle by which culture is transmitted (and with it ethnocentrism). Much of the interaction sociologists study consists of the exchange of words. And language is itself shaped by the social process which it helps to shape. "Words are what make us human," says Chase. "At the same time, words are full of traps, distorting evaluation, leading to pain and misery beyond all sense and reason" (p. 288).

Chase sees no escape from this paradox, but suggests that the appropriate attitude toward it is appreciation "for the power and utility of words, and determination not to misuse the gift. Words are like a sharp new ax, invaluable for the pioneer as long as he does not let it slip. Today, however, sharp axes are flying in all directions" (p. 288).

The sociologist, at least as much as the man in the street, must know how to cope with the barrage.

WILLIAM R. CATTON, JR.

Reed College

Caste in Modern Ceylon: The Sinhalese System in Transition. By BRYCE RYAN. New Brunswick, N. J.: Rutgers University Press, 1953. ix, 371 pp. $6.00.

It is a pleasure to report that this is as fine a sociological document as it has been this reviewer's privilege to read in many a year. The author, a Professor of Sociology at the University of Ceylon, is thoroughly knowledgeable about the best theory in the field, and his eye and pen are as keen and bright as one could hope for. I do not know any study of a social system in operation which is more sophisticated, balanced and persuasive than *Caste in Modern Ceylon*.

If one were to choose the most difficult kind of social system to observe and report systematically, certainly a caste system in transition would be a contender for top honors. Yet somehow Dr. Ryan has managed to convey the full flavor of the rock-like stability of caste, and at the same time, in all their meaningfulness, both the major and minor fluxes of the system as it moves toward westernization and secularization.

The volume opens with a most illuminating summary of the Sinhalese caste system, and then turns in Part I to the Institutional and Historical Backgrounds. The three chapters of Part II are devoted to a systematic rendering of the formal structure of the caste. Part III then focuses on the caste system in the villages in operation, and in part IV the same types of observations are made about Caste in the New Society. The Introduction and Chapter 13, A Social Structure in Transition, would make splendid readings in any anthology of social organization.

The focal point for the transition from caste to class and from eastern to western ways of life is found in the city of Colombo, whose metropolitan area contains some half million persons, and which stands out as "the center of dominance, governmentally, economically and educationally." Here one sees class and caste distinctions operating side by side, in a fashion which, though apparently incongruous and incompatible, according to simple-minded structural-logic deductions, is nevertheless very much alive and functioning.

Professor Ryan's words are better than any summary I could provide. "Class differences are far more visible and probably more objective and sharper than in the Western nations. In manner utterly incomparable with the class structure of western societies, the fundamental status division of the urban population is into an English educated, shoe and trouser wearing, white collar and professional upper class, and the saronged, barefooted, vernacular-speaking labor class. This is the most highly visible status division of the society. . . *With it caste bears no conformity.* . . The fact is that caste has not disappeared from the city. Although many traditional aspects of the system have died away, the deep sentiments which still support the endogamic principle in marriage have been partially redirected and transformed. In an era of democracy and pleading for national loyalty above 'communalism,' many behavioral manifestations of caste have 'gone underground.'"

The author indicates how strong is the survival of the endogamic principle, even among the sophisticated, western-minded university students, among whom it is absolutely wrong form even to mention a man's caste much less to raise questions regarding caste-membership or to employ caste membership as a criterion of eligibility for social relations.

Of great interest is Professor Ryan's analysis of the manner in which and reasons why caste is more stringently observed among the secularized, westernized, upper-class, economically-secure, English-speaking Sinhalese than among the barefooted coolie laborers and skilled artisans, in whose ranks fall most of the Sinhalese population, and among whom "it is probable that the greatest biological mixing of the castes is taking place today." The disrupting influences of urban life, most especially the economic hardship and marginal existence in the city, and the proliferation of non-caste-defined occupation, are placed in proper causal context in this process of disintegration of caste. Equally impressive is Dr. Ryan's treatment of the symbolic and sentimental reinforcement of caste lines in the interior jungle and highland areas, and low-country towns and villages.

Space forbids more accolades, merit them though the book does. Suffice it to say that the reviewer, who has tried his hand at observation and reporting of a relatively simple village in a study of caste in transition, can have nothing but deep admiration for Dr. Ryan's excellent study of a much larger and more complex social system.

MELVIN TUMIN

Princeton University

Amerika Mura: Imin Sōshutsu Son no Jittai (Inflence of Emigrants on their Home Village: Report of a Survey of Amerika Mura). By TADASHI FUKUTAKE. Tokyo: Tokyo University Press, 1953. 508 pp. $2.20.

This volume is a detailed report of a study made in 1951 of an isolated fishing village in Wakayama prefecture under the sponsorship of the Population Problems Research Council established by the Mainichi newspapers. Twenty-two persons selected from the staffs of four universities and six government agencies participated in this survey which was designed to throw light on emigration as a possible solution of post-war population problems.

The village surveyed became known as Amerika Mura because of a stream of emigrants to the Fraser river valley of British Columbia who for more than 50 years maintained close contact with their relatives left behind and contributed liberally toward their support. So extensive was this migration that three-fourths of the village families have either emigrants or ex-emigrants, and at one time the 1500 residents of the village were equalled in numbers by the emigrants and their families in Canada. While emigration does not always relieve population pressure, in this village the size of family decreased from 5.2 in 1901 to 3.7 in 1951. The village birth rate in 1950 was 15.1 per 1000 in contrast to 28.3 for the nation as a whole.

Economically, the village, which had for

generations been poverty stricken because of very limited resources, prospered by remittances from abroad. Concrete roads were built and some of the houses became more western in appearance and equipment. Clothing was to a considerable degree westernized, the diet included western articles of food, and sanitation was considerably above Japanese rural standards. The patriarchal type of family largely disappeared and there was much less emphasis upon the usual distinction in rural areas between head and branch families. The old religions, however, maintained their sway over the people, and Christianity apparently exerted no influence upon the community.

On the whole, the survey showed that the emigrants did not tend to become assimilated in their Canadian environment but always looked upon their native village as a place of retirement in their old age. Their Canadian-born children were as far as possible sent back to the village for at least a part of their education, and they tried to bring them up so that they would be equally at home in both countries. During the late war the villagers, cut off entirely from contact with their relatives in Canada, suffered severe privations. The repatriation of many of those abroad after the end of the war brought an increased burden upon the village. Nearly 600 Nisei are eagerly awaiting an opportunity to go back to British Columbia, and the Canadian Liaison Council in the village is doing what it can to reestablish the emigration movement which in the past brought prosperity to the community. But as the study clearly shows, the village profited by this emigration because the emigrants remained loyal to their homeland and failed to become thoroughly assimilated to their new home abroad.

The significance of this survey is by no means limited to the factual data presented and the conclusions set forth in the published report. It stands out also as a venture in cooperative, interdisciplinary research of a local community situation utilizing quantitative materials tabulated and analyzed in accord with modern statistical techniques. In contrast to the pre-war emphasis in the field of sociology upon abstract problems of a philosophical nature, this study of Amerika Mura points the way to a more fruitful development of research of Japanese institutions.

That this volume represents a trend in this direction is borne out by the 1953 program of the Japan Sociological Society. Out of the 90 papers presented at that meeting, only 18 dealt with theoretical topics. This growing emphasis upon social research has been greatly advanced by a group of young rural sociologists who with limited research funds at their disposal have carried on field studies usually of more isolated mountain villages in order to discover the extent to which old customs and traditions still survive. The rural sociologists, recently organized into a society of their own, held their first annual meeting in the autumn of 1953 in cooperation with the agricultural economists and discussed problems of land reform which indicates an increasing emphasis upon actual living conditions and problems of rural people. The recent translation of George Lundberg's *Social Research* by Professor Fukutake and its publication by the Tokyo University Press is further evidence of a growing interest in concrete studies and may be a significant step toward a closer relationship between Japanese and American sociologists.

JESSE F. STEINER

Arlington, Washington

Economic Anthropology: A Study in Comparative Economics. By MELVILLE J. HERSKOVITS. New York: Alfred A. Knopf, Inc., 1952. xiii, 551, xxiii pp. $5.75.

Economic Anthropology is really a second edition of *The Economic Life of Primitive Peoples* published in 1940. It has been expanded with materials from the extensive new literature on the economics of primitive people, and by some reworking of the theoretical chapters. Though it is a second edition, it deserves a full review, since it is the sole work by an American anthropologist dealing with primitive economics.

An unusual feature of *Economic Anthropology* is the inclusion of the review of the earlier work by the economic theorist, Frank H. Knight, together with Herskovits' rejoinder. Knight attacks Herskovits for his empiricism and makes a plea for the deductive approach in social science and the use of model analysis. His objections are as much directed at "institutional economists" and sociologists as at Herskovits. They also cover an attack on a sort of social welfare point of view as distinct from "hard-headed" classical economic theory. The rejoinder is a denial of the validity of "economic man" and an insistence that economic analysis must use the real world of primitive life for its analysis; it is a sharp cry for empiricism. I mention this at some length, for in the failure really to lock horns lies a clue to the essential weakness that I feel mars Herskovits' work.

For, though we agree that economics builds theoretical systems on a case of one (or at

least on a highly selected set of cases), and that the concept of "economic man" is an anthropomorphic reification of outmoded psychological notions, yet we are not given an adequate theoretical substitute. Herskovits' rejection of classical or neo-classical economic theory is not the problem; rather it is his rejection of all theory. Theory, that is, in the formulation of workable hypotheses of systematic interrelationships and their testing in the data at hand. The remainder of this review will be a critique first of Herskovits' anthropological position and second, of his understanding of economics as an aspect of human social behavior.

Herskovits belongs squarely in the Boas tradition: elevate the factual and evidential; minimize theory and preconceived hypotheses. All ultimately is culture, and culture is free to develop as it will, unfettered by any laws—environmental, sociological or evolutionary. Now this anarchistic point of view may well be correct, but the ironic fact is that the only way to demonstrate its validity is to assume the opposite; Herskovits' method precludes the possibility that he will ever prove himself to be correct.

The position is so anti-evolutionary that it does not even consider proper the word preliterate, much less primitive; it denies that modern primitives give us direct clues to earlier forms of society, and it avoids any classification of societies. This leads to another irony. For Herskovits, by avoiding all developmental implications lumps together all non-literate societies and counterpoises them to industrial ones. As a result, the reader feels a unity in primitive societies despite Herskovits' explicit denials. His method is to cite cases at random so that, for instance, in the course of the first six pages of the chapter on "Business Enterprise, Credit, and the Determination of Value" he cites consecutively: Melanesia, Bush Negroes of British Guiana, pre-Spanish Mexico, Plains Indians, Pueblos (Taos), Melanesia (a second group), Yurok Indians of California, Hidatsa Indians, Northwest Coast Indians, Navaho.

Now it would seem to be the first order of business to invoke some taxonomy of economic systems among primitive people, but there is none. The most feasible and usual taxonomy is on a production basis established in the earliest evolutionary writing; perhaps this accounts for the avoidance. Nor is there any classification of types of distribution, of consumption, or of any other major economic feature. Yet surely in economic life it is reasonable to presume some order—some lack of uniqueness of each cultural solution—considering how closely economic life is tied to biological demand, natural environment, and technology.

On the economic side, Herskovits treats aspects of the economy separately rather than as a system of interrelationships. We can see this in the general organization of the work. Each chapter treats with an economic topic. After an introduction, there are chapters on production (getting a living, patterns of labor, incentives and reward, division of labor), on exchange and distribution (gifts, trade, business enterprise, consumption and capital formation), property (ownership, land tenure and goods) and on economic surplus (population size and social leisure, government, the supernatural, and display). Each topic is illustrated with data from many tribes (as already noted) to show how differently matters are handled in different cultures. The result is that each subject item is seen as a discrete thing—a sort of annotated dictionary of economic concepts as they apply to primitive—i.e., nonliterate—societies.

But no science is merely a set of concepts; rather, it assumes that there are dynamic and predictable interrelationships between separate phenomena. Such relationships cannot be sought if we examine, say money in chapter XI and land tenure in chapter XV and labor in chapter V. If Herskovits does not believe that such regularities exist in economics, he should either avoid the systematic treatment of it, or should set out systematically and forthrightly to demonstrate his position. But because he does not examine systems he cannot do this.

Now Herskovits is correct when he says that present economic theory applies only to industrial civilization. What is needed is a broader frame of reference in economic theory; broader generalizations regarding man's economic behavior which will fit the cases found in the literature of anthropology, and in which industrial economic systems find their place as one special case (or several). This, however, he cannot do, because he does not admit of taxonomies of economic forms, regularities in social systems, or the possibility of laws of societal behavior; nor does he see economics as a system of interrelated phenomena. Or, rather, he reduces economics to anthropology, makes everything culture, *sui generis*.

This review has been directed to the implicit theory regarding economic and social behavior; it is admittedly written from certain intellectual predilections. It would be highly unfair not to give Herskovits the much credit he is due for bringing together so much data on

primitive economic life, for orderly presentation and clear exposition, for a valued textbook for courses in primitive economics, and for a reference work.

WALTER GOLDSCHMIDT
University of California, Los Angeles

Amazon Town: A Study of Man in the Tropics. By CHARLES WAGLEY. New York: The Macmillan Company, 1953. xi, 305 pp. $5.00.

Although the principal primary data on which this book is based come from what the author calls a "case history" of a small county-seat town of about 500, near Manaus, and some analyses including the outlying neighborhoods, much space is given to general historical or ethnological description of the Amazon region and of Brazil. "Case studies" were made of 113 families covering details of diet, expenditures, income, personal possessions and "other specific economic and social information." Sampling procedures are not described which makes the comparison of the race composition of class groups, averages and percentages questionable. The sociologist also may find the lack of the description of specific clique and other groupings a shortcoming. The investigators have done the generalizing for the reader.

Sociologists will find the monograph very readable and interesting and from the reviewer's point of view, it is a superior ethnographic report. For the specialist and general reader alike much of its interest derives from the fact that it treats "the most extensive sparsely inhabited area of the world" (p. 291), which many have considered under-populated, and it deals with problems involved in introducing improved technology and change.

Judging from space and emphasis given by the author the following seem to be the chief characteristics of the area as presented in the monograph:

(1) In politics, business and other activities the most important reference group (called institution by the author) is the family and its extensions through the *compadresco* or ritual relationships such as god fathers, mothers, etc.

(2) A "fixed system of socio-economic classes" emphasizing ascription and stressing one upper class of whites and three lower classes of lesser town dwellers, farmers, and island collectors. These are all results of the early slave system and later creditor-debtor relationships which control relationships throughout the area. The author quotes the general rule of thumb for Brazil as applicable for the village under study. "The lighter the skin, the higher the class; the darker the skin the lower the class" (p. 131). In the area of study, Negro ancestry carries higher prestige than Indian ancestry but "prejudice and discrimination based on race are mild. . . . (whereas) In Sao Paulo, in Rio de Janiero, and other industrial centers discrimination seems to have increased in recent years" (p. 263).

(3) The heritage of "boom and bust" of the rubber and other extractive activities involved in gathering products for sale, the "strike it rich attitude" and disparagement of physical labor has made agriculture for food and fiber, which is based upon the "slash and burn" system, primitive and neglected.

(4) Communication and transportation are poor or lacking.

(5) Until recently health conditions and facilities were among the poorest.

(6) About 60 per cent of the people who inhabit the Amazon region are illiterate and the educational facilities are inadequate, or lacking, especially in outlying places.

(7) Modern technology and economic developments have made almost no contribution to the area and levels of living are very low.

(8) Formal religion and even the unofficial religion, which includes a surprising amount of play, ritual activities and fiestas and is prohibited by the church, serves to give the community solidarity and is especially important in tying the outlying villages together and to the center. However, town and country relations are filled with friction and ill-feeling, even though in Brazil, as in New England, rural and town portions of trade-center communities belong to the same political units.

(9) Magical and non-rational action from Indian, Negro and Portuguese sources pervade activities involved in making a living, medicine, and most other activities.

(10) Personal cleanliness and frequent bathing is the rule. The people are cleaner but poorer, more ignorant, isolated and less affected by technology than the Americans described by James West in *Plainville, U. S. A.*

(11) "Hospitality, almost to a point of ostentation, is a traditional Brazilian trait, and the elaborate politeness and polished manners of the aristocracy are characteristic also of the simple country folk" (p. 264). Generalizations concerning the strategy of change, such as the following, are made throughout the book:

(1) "A new idea will be accepted only when a basis is present in the preexisting culture to make the element a useful one" (p. 18).

(2) "In opposing the neighborhood brotherhoods and festivals, the Catholic priest and the official Church organization are combatting an important social institution, unaware of its great potential value to themselves and to the community" (p. 213).

(3) "Yet when a culture, through lack of tech-

nological equipment and for reasons of social organization, fails to provide for the material needs of man beyond a mere survival level, that society and culture must be judged inferior" (p. 295).

(4) "But the main factors which make for the difference between those communities which furnish only a bare minimum existence and those which provide more amply for human needs are, in a broad sense, social and cultural" (p. 287).

CHARLES LOOMIS

Michigan State College

Approaches to Community Development: A Symposium Introductory to Problems and Methods of Village Welfare in Underdeveloped Areas. Edited by PHILLIPS RUOPP. The Hague: W. Van Hoeve, Ltd., 1953. xvi, 352 pp. No price indicated.

Approaches to Community Development is a symposium by twenty-two persons from eight different countries. Most if not all of the authors have apparently had some connection with community development projects in the so-called undeveloped areas. The book has the defects of most symposia in that the various essays are not of uniform quality, although the editor tries to tie the contributions together by an overall "central purpose which orders and unites its parts."

In the Introduction the editor, Mr. Ruopp, sets the philosophical framework of the volume by a discussion of historical thinking about the concepts of "community" and "development"; and Mr. Hart, the Executive Director of Community Projects, Ltd., writes of the role of non-governmental agencies in the field of community development. The rest of the volume is divided into four parts. The first three are devoted to analyses of "The Sociology of Community Development," "The Economics of Community Development," and "Education for Community Development." Each of these parts opens with a chapter on the aims, implication, or background of the subject under consideration. In two or three other chapters some specific aspects of the subject are elaborated, such as Social Values and Community Development, Land Tenure and Land Reform, The Concept of a Team, etc. The last chapter of each of these parts tries to summarize all the details in a so-called case study. In the last part, entitled "Regional Considerations," some of the problems of Africa, the Arab Middle East, India, Indonesia, and Latin America are discussed in separate chapters. Throughout the book generalizations are made concerning the common problems encountered and methods found to be efficacious in solving them.

A careful reading of this book should assist those who are now or may later be working in undeveloped areas to help people help themselves. The more general reader will be impressed by the enormity of the perplexing problems which are met and the difficulties involved in keeping a community integrated as changes are advocated and brought about. The authors point out that aiding undeveloped areas has to be a long, slow process; otherwise a community loses the very cohesiveness that holds it together. Much of value can be learned from this volume which can be applied in communities which are not usually considered undeveloped.

In this reviewer's opinion, however, the editor and authors would have succeeded much better in their common endeavor if they had supported their generalizations by describing in detail the work each had been carrying on in the separate communities. Although it might be supposed that the case studies at the end of each part would do this, they do not. The book is abstract, not to say abstruse, and is not "reality oriented." The abstractness plus the complexity of some of the writing makes this a hard book to read.

H. ASHLEY WEEKS

New York University

Human Communities: The City and Human Ecology. By ROBERT EZRA PARK. Glencoe: The Free Press, 1952. 278 pp. $4.50.

The second in a series of three volumes of the collected papers of Robert E. Park, *Human Communities,* affords a later generation of students with a sense of the vigorous intellectual ferment which stirred in the mind of one of America's great sociological pioneers. Although the nineteen articles, prefaces, and reviews which make up this volume were selected by the editors as reflecting Park's thinking about the city and the emerging field of human ecology, it is apparent in every chapter that he gave a broader interpretation to these fields of study than is common today. Park's earlier experience as a newspaper man and his insatiable interest in human nature would not permit him to restrict his analysis of the community to its purely measurable aspects, although he readily accepted as the ultimate objective of social science, "the reduction of the colorful variation and detail of human experience to the dull uniformity of a mathematical formula."

It was, however, Park's concern with the social and cultural diversity of the modern city and his desire to find some order and regularity in its apparent chaos which led him to pioneer in human ecology. One of the first, if not the very first, direct references to human ecology

in sociological literature is to be found in Park's paper on "The City: Suggestions for the Investigation of Human Behavior in the Urban Environment," published in March, 1916. Proceeding from his widely cited thesis that the city is "a state of mind" rather than "a mere constellation of institutions and administrative devices," Park soon arrived at the conclusion that the city's culture was rooted in the soil and that one of the most effective means of isolating and describing the typical configurations of city life was on the basis of its physical structure. Hence, Park's persistent interest in the processes by which both people and institutions are distributed in space.

Herein also lies the explanation of Park's double concern with human ecology—first as a scientific discipline to be cultivated for its own sake, and second as an index or clue to the life and culture of the community. Even in the eight papers devoted to the concepts of human ecology, Park invariably calls attention to the two levels of human society—the biotic and the cultural, with the cultural superstructure resting upon a symbiotic substructure. While emphasizing the central significance of competition within the ecological order, Park insisted that "in human society, competition is limited by custom and culture." Even such a basic ecological concept as dominance is conceived by Park as functioning on the political and cultural levels, as well as on the biotic and economic. In general, the analyses of the city in terms of the abstract symbols on a map and the other devices of human ecology are valued chiefly for the light which they shed upon the people who live within the city and upon the order which binds them together.

What is perhaps most impressive in the rereading of these early papers on the urban community, is the wealth of suggestive ideas whose research possibilities have not yet been fully exploited. During his lifetime, Park manifested a phenomenal capacity for stimulating younger men to follow out the research ideas which he had first suggested. This is clearly revealed by the inclusion in this volume of such a large number of his introductions to research monographs by former students. But Park's ability to develop research hypotheses far outstripped the capacity of his students to subject them to scientific testing, and so it is that nearly forty years after the appearance of his first paper on the city, there remain in this volume scores of discerning ideas and hypotheses regarding the community which deserve further examination.

Park always placed a higher premium in his own writing upon penetrating insights than upon rigid adherence to any of the methodological rituals of science, and virtually the only point in the entire book at which he appears somewhat impatient with the work of a fellow social scientist is in the one chapter—previously unpublished—dealing with an unimaginative but rigidly statistical approach to the study of the city. But even in this article, Park did not wish to minimize "the importance of the statistics and correlations which had been gathered at such expense of time and patience." He insisted only that they raised more questions than they answered and he urged instead "something less precise perhaps and more empirical than the statistics and the formulas on which the conclusions in this study are based."

At a time when sociological research is conceived so largely in terms of methodology and the application of purely quantitative techniques, Park's insistence upon insight and understanding as prerequisites to scientific knowledge comes as a wholesome and necessary caution. Human ecology, as Park rightly points out, may contribute significantly to the development of sociology as a science through its quantification of social distance in terms of physical distance, but the sociologist must constantly bear in mind that "space is not the only obstacle to communication and that social distances cannot always be adequately measured in purely physical terms."

ANDREW W. LIND
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Urban Behavior. By E. GORDON ERICKSEN. New York: The MacMillan Company, 1954. xiv, 482 pp. $4.75.

This book is another addition to the now relatively long list of recent books intended for use as texts in urban sociology. As such, the author claims no major contribution to urban theory, and, appropriately, claims to have written for the student rather than the scientist. These two aims seem to have been relatively well carried out in the book. The style is readable and visual aids are used in moderation.

In theory, the author considers the rural-urban dichotomy obsolete and substitutes the idea of a rural-urban continuum. This is in keeping with current thought in the field. He offers brief definitions of the poles of this continuum which are less adequate, seemingly, than those given by Robert Redfield in his formulation of the folk-urban continuum. Though Erickson discusses Redfield's continuum, it is not clear that he draws any distinct line between "folk" and "rural." If not, of course, the logical end of his continuum should be folk rather than rural. The continuum is undoubtedly a more useful concept than the rural-urban dichotomy. However, Redfield's folk-urban continuum concept re-

cently has been ably criticized by anthropologists, and it is probable that the continuum idea will be found inadequate by sociologists also.

Part I, which is entitled, "The City and Civilization," does a good job of presenting the author's point of view and of defining concepts. The history of urbanism is covered in one chapter in this section. This chapter is fairly good but is a very minimum outline of the subject. A chapter on "History of Thinking About Rural-Urban Life," also included in this part, seems to be a very worthwhile contribution.

Of importance, to many, is the heavy weighting given to human ecology in this text. Part II, which is devoted entirely to this subject, takes nearly 200 of the 467 pages in the book. This section is well done in the sense of being a good summary of the research findings on urban ecology. The author's point-of-view is fundamentally that of conventional ecology. Much material is included which is normally included in the introductory course in human ecology. For students not having an opportunity to take a special course in that subject, this section might be a welcome substitute. In reality, this section may well be regarded as the main body of the book.

The social structure and social psychological aspects of the urban community are dealt with briefly in the fine, relatively short chapters of Part III. It appears to be the purpose of this section to give the student just enough information regarding the basic institutions, the occupational structure and the class structure to orient him to the urban community. The section falls far short of giving a comprehensive treatment of the group structure of the city. Part III receives barely twice as much space as is devoted to the subject of land use alone in the section on ecology. The sociological aspects of the city seem to be deserving of more emphasis than this in a book intended for use as a sociology text.

One of the strong points in the book is the emphasis placed on social process and social change. Sociological "still shots" are not only unrealistic but are less interesting to the student than social change. The emphasis on process throughout the book makes Part V on city planning a logical climax. A relatively good but somewhat brief treatment of the subject is given. It is perhaps sufficient to give the student a layman's understanding of the process and of what may be accomplished through planning.

HARLAN W. GILMORE

Tulane University

Transportation and the Growth of Cities. By HARLAN W. GILMORE. Glencoe: The Free Press, 1954. vi, 170 pp. $3.00.

The fundamental importance of transportation is reflected in the already large volume of monographic and other literature on the subject. But this is the first book, so far as this reviewer is aware, devoted exclusively to a sociological analysis of the effects of transportation changes on urban development. It is not unreasonable to hope, therefore, that it will make a substantial contribution to knowledge about the growth of cities.

In the first three chapters the author quickly traces the development of transportation from the ancient to the modern period. Special attention is given to the stationary wagon axle, the methods of harnessing animals, to the tacking sail ship, and to the types of metal used in railroad trackage. Each chapter concludes with a few paragraphs on implications for urban development. The implications pertain, of the most part, to the relationships of cities with rural areas, three types of which are stressed: (1) taxation of rural areas by cities; (2) trinket trade, or the exchange of city-made non-essentials for the essential produce of rural areas; and (3) equal trade between urban and rural areas. The social systems associated with the three types of economic systems are described in chapter four, with reference particularly to standards of living, urban-rural relations, power structures and status systems, and value systems. At this point the discussion turns to the distribution of retailing and wholesaling functions among villages, farm market towns, collection cities, and terminal cities. Passenger travel inventions and their effects on the locations of functions within cities are also considered. And, finally, the author analyzes certain rural-urban traditions and sterotypes against the background of transportation change.

Although it is the author's belief that "transportation systems are a better key to socio-economic systems than social scientists have recognized," his book falls short of a satisfactory exposition of that conviction. Even granting the author's narrow view of transportation as including only vehicular movement, his treatment skims lightly over the complexities of the matter calling attention here and there to the presence of a relationship between selected variables but seldom probing sufficiently to illuminate the relationship. Perhaps the fault lies in trying to span so large a subject in so small a book. In any event, the mere juxtaposition between two covers of materials on transportation, economic systems and social

systems does not fashion a key to an understanding of their interdependencies.

Despite the failure of the book to serve the need of professional sociologists, it should appeal to undergraduate students and possibly to adult laymen. Its interesting factual content is simply and lucidly presented. And it at least calls attention to a hypothesis that has wide ramifications.

AMOS H. HAWLEY
University of Michigan

Introduction to Malthus. Edited by D. V. GLASS. New York: John Wiley and Sons Inc., 1953. x, 205 pp. $2.75.

This book consists of three "introductory" papers intended to provide some background for the study of Malthus as well as to introduce the public to his ideas; a bibliography "intended to cover the genesis and course of the Malthusian controversy in Britain" (Godwin 1793 to about 1880); and two papers by Malthus not generally available today.

The first of the introductory papers, "The Historical Context of the *Essay* on Population," treats primarily of the effects of the Essay upon the reform of the Poor Laws which effects the author (H. L. Beales) does not approve. The author probably did not intend it but he leaves the impression that the truth of Malthus' ideas is to be judged by the conclusions drawn from them by men for practical purposes in the few decades after the publication of the Essay. At the end he asks "What, then, did the Malthusian Theory of Population contribute of value to the making of social policy?" This seems to the reviewer to ignore completely the more significant aspects of Malthus' Essay.

The second paper, by the editor of the book, deals in the first instance with Malthus' idea of *moral restraint* as a repressant of population growth which was elaborated in the second and subsequent editions of the Essay. It stresses the fact that Malthus himself never approved of artificial and unnatural modes of checking population, both on account of their immorality and their tendency to remove a necessary stimulus to industry. The reviewer has never been able to find out to just what practices Malthus referred as "artificial and unnatural." Apparently, the author is also in the dark on this point. The body of this paper then goes on to discuss how Malthus' *moral restraint*, primarily the postponement of marriage not infrequently leading to confirmed bachelorhood, has become a fairly effective method of population control in Ireland, although not the only factor. He also mentions other attempts, particularly in Germany, to influence population growth by postponement of marriage and restrictions on marriage.

The third paper (Alan T. Peacock) tries "to divorce the Malthusian theory of population altogether from its political and social background and consider it as an economic analysis." In this the author has been quite successful. I cannot do better than to quote his own summary of truth in Malthus' argument, "To sum up, I think it can be said with some force that the Malthusian theory of population does at least give prominence to the relevant factors which influence economic development in the broad sense, even in the present century. Consequently, it is churlish to dismiss him as a second-rate economist and sort of amateur anthropologist. It is true that it may not be possible to accept that there is any automatic link between the level of subsistence and the birth-rate and that what is left of the theory may amount to a truism. Nevertheless, in the next decades, in view of the imminent decline in mortality rates in under-developed countries, 'Malthusian Devil P of Population' may be a constant danger."

Little can be said about Malthus' own papers. His "Summary View" should be read by all who do not have time to read the latest edition of the *Essay* if they would understand Malthus' "Principle of Population." But even this is inadequate. It is to be hoped that it will stimulate some readers to go to the Essay. The second paper, a letter dealing with the population consequences of a proposed reform of the Poor Law, may be regarded by many as only of historical interest today. To the reviewer it seems also to place in a clear light the great difficulty to be encountered in raising the level of living in a population where only a slight improvement in the per capita production can reasonably be expected from year to year but where it is reasonably certain that such improvement as does take place will result in a reduced death rate some years before its effect on the birth rate becomes visible.

WARREN S. THOMPSON
Scripps Foundation for Research in Population Problems

Population Problems: A Cultural Interpretation. By PAUL H. LANDIS (Second Edition prepared by PAUL K. HATT). New York: American Book Company, 1954. xiv, 554 pp. $6.00.

The rapid accumulation of research and the development of new knowledge makes the frequent revision of standard textbooks a highly desirable practice in all social science fields. There are few areas, however, where this

necessity has appeared as pressing as in the case of books dealing with population problems. All population texts were rendered obsolete not merely because new census materials became available but also because they were written at a time when Western populations seemed on the verge of imminent stagnation and decline. The rising birth rate of the 1940's and 1950's, the continuing increase in life expectancy, and other demographic changes associated with war and postwar developments have sufficiently changed the situation to make new interpretations of the data necessary. It is therefore no exaggeration to point out that the second edition of this popular textbook was awaited with impatience.

Contrary to customary practice this revision was prepared not by the original author but by the late Paul K. Hatt, who was apparently given this assignment because of disagreements between author and publisher. The reviser set himself a threefold task: (1) to incorporate the latest data into the book and bring it up to date statistically; (2) to make changes in interpretation dictated by the events of the past decade; and (3) to reorganize the material in order to improve its value as a teaching instrument.

On the whole it seems to this reviewer that Mr. Hatt has well accomplished what he set out to do. The original merits of this text—its primary emphasis on the social and cultural factors which influence the biological behavior of man, and the consistent use of a sociological framework for the interpretation of demographic phenomena—have been admirably preserved. The new materials have mostly been incorporated into the previous lines and paragraphs so that the individual chapters are brought up to date without drastic revisions.

The organization of Part I "Population Facts and Population Theories" remains unchanged, but care has been taken to make at least brief mention of Marxian theory and of the "Optimum theory" of population which had been unjustifiably omitted in the first edition. Part II is now called "Cultural and Demographic Factors in Population Replacement" and contains the chapters on sex and age composition in addition to the materials on mortality and fertility trends. The analysis of mortality and fertility differentials has been separated from the discussion of the trends and now forms Part III "Differential Population Replacement." Part IV "Structural and Spatial Distribution and Movement of Population" contains the chapters on race and nativity composition, residential and regional distribution, and occupational and industrial composition, as well as the two chapters on internal migration. In Part V "International Aspects of the Demographic Position of the United States, Problems and Policy" we find the two chapters on international migration, two chapters on population policy, which, widely separated in the first edition, have now been sensibly brought together, and a new chapter "The World Demographic Position of the United States," written by Hatt especially for the second edition. The addition of this chapter was explicitly designed to counterbalance the almost exclusive focus on the population of the United States which is characteristic of this text. However, the battery of tabulations of comparative international statistics and the accompanying sparse textual interpretation hardly remedies this defect. It impresses the reader as a make-shift device and a tacked-on half measure. One is also amazed to find incorporated into this new chapter charts presenting international data on infant mortality and maternal death rates which, together with the accompanying text paragraphs, are based on 1938 statistics and have been adopted unchanged from the first edition, although current statistics at least for infant mortality are readily available.

The second edition features one further innovation, a special appendix on "Essential Statistical Concepts and Methods in Demography," written by Leo Silberman. Although it is somewhat uneven in the treatment given various concepts as, for example, the method of Life Table construction, this appendix represents a welcome addition. It contains a valuable and well-written, if brief, discussion of the main demographic methods and statistical concepts which have become indispensable tools not only for the working demographer but also for an adequate classroom presentation of population problems.

KURT B. MAYER

Brown University

The World's Food: A Study of the Interrelations of World Populations, National Diets, and Food Potentials. By M. K. BENNETT, New York: Harper and Brothers, 1954. vi, 282 pp. $4.00.

"Tell me what a people eat, and I will tell you how they live." The phrase has a ring of truth that has led to its too ready and uncritical acceptance. Some demographers have added the words "and die." Either way, affirmation has been general. Anyone who carefully reads *The World's Food* is unlikely ever again to view a statement of this kind except as a bit of braggadocio, or at best, as a suggestion for a series of complex research projects. Mr. Ben-

nett by painstaking and discriminating use of empirical evidence and of logical reasoning has made it clear that the relations between diet and health, and between food and population growth, are highly complex and largely unanswerable at the present level of nutritional research.

Mr. Bennett is a specialist in food economics. For over thirty years he has worked with the Food Research Institute at Stanford University and is presently its Director. He describes himself modestly as a layman in many aspects of nutritional research. After such a statement I can only admit my own total ignorance of the field and refrain from attempting any critique of the materials on food consumption and ingestion which makes up all but the first three chapters.

I can nevertheless indicate that these materials involve a detailed examination of the American diet (Part II) and a more general view of national diets in peace and war in other parts of the world (Part III). They appear to be a cautious and realistic appraisal of what we know and do not know about the food available to and used by men and the value of that food for supporting life and maintaining health. Various data and conclusions in current use are sharply challenged, including some which undoubtedly gain in authority by their sponsorship by such agencies as the Bureau of Agricultural Economics or the Food and Agricultural Organization of the United Nations. A number of items of folklore are effectively demolished, for example, that there are scientifically established or establishable nutritional requirements for individual commodities (p. 94); that estimated per capita calorie requirements of one country may be meaningfully compared with those of another (pp. 102–108); or that satisfactory evidence of malnourishment is provided by the nutritional survey "which, after ascertaining what the surveyed sample population purchases or eats, compares this with standards or allowances concerning what hypothetical people ought to eat, leading to conclusions that malnutrition must exist in the surveyed population if it does not ingest what is recommended for somebody else" (pp. 205–206).

The direct value of the book to sociololgists lies mainly in the area of population study. Except for a few scattered references, demographic materials are confined entirely to the first three chapters, which were initially given as public lectures at Rice Institute, and which, in my opinion, are more a separate treatise than an integral part of the book. Chapters 1 and 3, dealing with population growth, past, present, and future, are a careful and informed summary presented with a modesty which is refreshing after some of the more vivid writing in this field. Chapter 2, which describes available foods and conditions of famine in the world since the Middle Ages, will prove more informative for professional demographers. Mr. Bennett concludes that during this time the variety of food intake has greatly increased for many peoples and that calorie consumption per head has tended to rise with the upsweep of numbers. Famine of course has largely ended except for special causes, mostly political. Quite properly, Mr. Bennett explains these conditions in terms of *over-all* economic development, of which they are facets, rather than as consequences of specific events such as additional land under cultivation.

These chapters will provoke some demographers into queries, not I think over facts, but over their interpretation. Changes in vital rates, like changes in food consumption, are also attributes of general economic development, which is a social as well as a narrowly economic process. This point is not clearly made. Rather, unwary readers may be lured into accepting "independent" causes of changes in vital rates (*e.g.*, p. 47). Further, although I fully concur with the principle that resources are not finite and that the possibilities of support for larger numbers of people at higher standards of living through further technological innovation and application are great, I should prefer to see some consideration of the existence of an ultimate limit for population in specific regions at specified levels of living. Personally, I doubt that such limits will frequently be reached in practice among populations which have achieved any substantial economic advancement. Therefore, discussion of the possibility of supporting additional numbers by achieving rising consumption levels everywhere (Mr. Bennett regards this only as possible in a given time period, not as *probable*) may be academic. There is no discussion of the contribution toward increasing levels of living or of food consumption which might be made by a policy of checking population increment.

There are other points with which demographers may not agree entirely. Controlling population growth is perhaps made to seem too easy. Mr. Bennett's apparent acceptance of J. S. Davis' views on the birth rate in the United States, and the lack of any attempt to relate birth rates to birth order and, except for passing reference, to family size may provoke some disagreement. These objections, however, must be viewed as minor alongside the generally excellent summarization of the population scene. They are certainly adequately

compensated for by the highly useful discussion of the world geography of hunger (Ch. 12). One valuable service here is the specific distinction in meaning made between the words "hunger" and "malnourishment." The simple act of defining these words in a meaningful way and the provision of specific refutation for some of the unacceptable conclusions in de Castro's *The Geograpy of Hunger* are among the major contributions of this chapter.

This is a critical study and the author has worked hard and consistently to get the facts and to use them intelligently. Because data are lacking we shall not find in this book solutions to the riddle of the real relations between food and population support. But we will learn that to get such answers, we shall have to devote less time to developing theoretical charts of food required and available and more time to observation of real conditions here and abroad.

VINCENT HEATH WHITNEY
Brown University

The Social Economics of Agriculture (Third edition). By WILSON GEE. New York: The MacMillan Company, 1954. x, 616 pp. $6.50.

This, the third edition of the well-known text by Professor Gee, brings his 1942 edition up to date with respect to statistics, legislation, and other developments in rural life. It keeps the same outline of the previous edition (Part One: The Setting of the Agricultural Problem; Part Two: Economic Elements; Part Three: Social Factors; Part Four: Rural Institutions) with the omission of one part devoted to "Some Political Problems" (The Farmer and Government, The Tariff and Agriculture, and Farm Taxation).

Although the chapter on Farm-Relief Measures in Part One is brought up to date it in no way makes up for the missing material on rural government, although agricultural tariffs and farm taxation questions receive less attention today than they did earlier. So many state legislatures continue to be controlled by rural counties, to the detriment of wise measures seemingly in favor of the more populous urban centers, that students in this field should be made intelligently aware of this problem. Then, too, the numerous problems of county government—in addition to those of education and health mentioned in the text—need the same serious attention given in the revised but not in this third edition.

The social science fraternity is fortunate in having someone like professor Gee who seeks in scholarly fashion to bridge the gap between the two disciplines of agricultural economics and rural sociology. One is impressed by the greater number of precise terms used in the economic section in contrast with the relatively few definite sociological concepts found in the treatment of social factors and rural institutions. For example, "social control" is mentioned in a quoted list of eight factors by which a rural community is identified but nowhere else is it defined or used in the analysis of rural life (p. 476). "Socialization" is quoted in a list of specific objectives sought through community organization (p. 479) as the best measure of organization in the community (after MacIver); and as one of the basic functions of the family (p. 518) but the term is nowhere defined in such a way that the student can use it as an approach to understanding the process by which the social heredity discussed in this text becomes a part of his own personality or style of life. Likewise, a number of changes in rural life are mentioned but no attempt is made to define "social change" and use it as a tool for analysis and description. "Social stratification" and "social differentiation" are both entirely absent. Until writers of rural sociology texts move even farther in the direction taken by Loomis and Beegle in their *Rural Social Systems*, the material dealing with agricultural economics will continue to give the impression of precision and definiteness (in spite of much disagreement within the field), while material in rural sociology will seem to be ameliorative and concerned with social problems rather than analytical and scientific. There are well-established concepts in sociology which deal with the heart of the discipline—social relationships, status-role bundles, social systems, or whatever one chooses to call them—and it is these concepts that must recur with greater frequency and be used in a well-defined manner in what seeks to be a sociological description.

The failure of Professor Gee to make use of such concepts is certainly due more to the tradition of writing about rural life to which he subscribes rather than to any lack of sociological insight on his part. This book is packed full of basic information, interestingly put together and lucidly written. Some chapters, such as that on "The Farm Family," are exceptionally well-done and should ensure the even wider adoption of this basic text which has set a high standard through the years.

IRWIN T. SANDERS
University of Kentucky

For A Science of Social Man: Convergences in Anthropology, Psychology, and Sociology. Edited by JOHN GILLIN. New York: The Macmillan Company, 1954. vii, 289 pp. $4.00.

The selection of the authors who contributed to this symposium would of itself have as-

sured a high level performance. The editor, John Gillin, has however come up with a gimmick calculated to give both sharp focus and unit of interest. He has taken two men from each of three disciplines and assigned to each the task of appraising the impact of one of the other two fields upon his own. In spite of a reported preparatory collaboration between participants, there is small evidence that it imposed any considerable constraint upon the authors. Each tackles the problem in his own characteristic way which the reader will be quick to identify. As sociologists, Talcott Parsons was assigned social psychology and Howard Becker anthropology; as psychologists, Theodore Newcomb assayed sociology and Brewster Smith anthropology; and as anthropologists, Peter Murdock deals with sociology and Irving Hallowell with social psychology. The result is a volume which is a measure of the vitality of these three disciplines, and where they are going.

The goal is, in the main, less ambitious than the title suggests. "For a Science of Social Man" is an engaging label for market purposes. It is a mark of the maturation—in mood, not subject-matter—of these disciplines lately arrived that they have worked through the stage of fetishistic clamour for the status of "science." The older traditional sense of science is appropriate in referring to what these authors aspire jointly to say. But with one possible exception they are concerned with the more specific areas circumscribed by limiting frames of reference.

In any event, the real concern and major impact of this volume is that it takes the three disciplines for whatever they are and concentrates upon the convergences between sociology, psychology, and anthropology. Nothing could be more relevant to the present state of affairs, and perhaps it was more fruitful to leave the task relatively unstructured. With the varied results in conceptualization, blueprinting and problemization, it will be clear that a notable degree of contact and continuity is achieved. The gimmick itself is partly responsible, but not all of these authors are noted for their mild-mannered deference to the other disciplines. Their job called for first, a workable restatement of the locus of boundaries, and second, an exploration of the working relationships, actual and needed, between them. With an occasional pardonable exception, these chapters exhibit little jurisdictional sensitiveness. In fact, it may sometimes seem that an author leans so far backward to credit the discipline he is relating that he belittles his own. The performance produces even some lighter moments when each of a pair extols the other for merits which to the other stand out as weaknesses.

The central job, however, is in locating convergences. The acknowledgment of dependence, documented in these essays, is the most obvious one. But it is the explicit spelling-out of their convergences that leaves the clearest picture of a series of disciplines developing as specializations while fertilizing and interpenetrating one another. First, by approaching the historical relationships between fields, the authors succeed in throwing into sharper relief the ways that interdependency brought about strains which in turn propelled substantive developments within each. What is more, compression of historical overview makes shambles of a good many disciplinary labels and claims to lineage priorities. It gets to be good serious fun when Murdock reads a whole "school" out of anthropology on charge of being sociologists, and Becker in turn insists upon assigning Murdock to the category of sociologists. Historically, it is clear that we deal with divergences as well as convergences, but with a persisting compulsion to interpenetration and with many of the conflicts flowing from temperament and expediency, not from necessity.

Second, having cleared the way, the road is open to pinpoint those central tendencies of conceptualization, theory—and perhaps jurisdiction—which are framing the agenda for each one of the three disciplines currently. Such convergences, it is clear, do not in these writers so much erase the lines between disciplines as articulate their limited fit in the area of man's behavior, and increase the interflow of resources by dependable communication. Out of their assessment grows the formulation at variable length of present and urgent problemization, not only within each of the three fields but in their demands upon the others.

This high level probing is provocative, demonstrating as it does the critical mood, the greater sensitiveness of communication, the diminishing parochialism, and the conceptual interpenetration of these disciplines. This certainly does not mean necessary agreement. For example, at least three different conceptions of social psychology appear: an applied field of psychology, a substantive focus of general psychology, an "interstitial field" between sociology and psychology, and even to forms of it called "microsology" and the other "psychology." What is significant is that all of them converge in dealing with phenomena of interaction, whether in Newcomb's lucid outline of a "new model" in which the behavior of persons is treated in terms of "the organization system of which these persons are parts"; or Parsons' focus upon the "organization of the stimula sit-

uation" as involving the social system; or Hallowell's reminder that anthropology goes back through Bastian to Herbart in its insistence that psychology cannot treat "man only as an isolated individual"; or Smith's call for a "new-old model" with minimal features which takes its start from the "interaction of persons."

This is a small volume of less than three hundred pages packed with historical perspectives and current conceptual evaluation. John Gillin has provided the framework and editorial direction for a book of such immediate relevance that its value to students of all levels in the social sciences is obvious.

WELLMAN J. WARNER
New York University

Sociology. By GEORGE A. LUNDBERG, CLARENCE C. SCHRAG, and OTTO N. LARSEN. New York: Harper and Brothers, 1954. xxviii, 740 pp. $6.00.

There is much that is admirable in this attempt to meet the somewhat conflicting needs of the variety of students typically found in beginning sociology courses.

The substantive topics covered do not deviate far from those dealt with in representative introductory textbooks. The most significant subject-matter omission is a chapter dealing with race and minority groups. A noteworthy inclusion is a chapter on mass communication and public opinion. The organization of materials differs from many texts however; for example, population and community are discussed before taking up the concepts of culture and society. Instructors wishing to follow other modes of organization will find their task facilitated by a system of paragraph numbering that simplifies cross-referencing to relevant discussions in other sections.

The book, dedicated to the students of two trial editions, clearly was planned to include features useful to the student. The table of contents, relevant segments of which preface each chapter, is an extended outline of the materials covered. The chapters close with summaries which, it must be said, do not always simply summarize. There is a profusion of well-chosen plates, charts, tables, and diagrams. The cross-referencing referred to above should prove useful to the interested and motivated student.

The provision of exercises in observation and analysis, as well as bibliographies and film references, may aid the instructor. He may also find value in the reading guide which keys appropriate selections from various collections of readings into chapters. However, the instructor who uses readings to show the student how the sociologist arrives at generalizations may find these unnecessary, given the space devoted to research tools, techniques and procedures, and the wealth of data found in this book.

Readers who are familiar with Lundberg's *Foundations of Sociology* and *Can Science Save Us?* will recognize the "tone" of this volume. Science is instrumental; it is the mechanism which permits the most adequate adjustment to environment, physical or social. The ends of science are prediction and control. These ends are best served by insisting on the essential identity of a science of the physical world and a science of the social world, and by following operational procedures in investigation. These themes, carried throughout the book, are presented in the initial three chapters, which deal with sociology as a natural science, science and language as means of adjustment, and problems of research procedure respectively.

One consequence of the foregoing is that the text remains close to the empirical level. The principles which it seeks to formulate are in the form of empirical generalizations. It should be pointed out, however, that the book is not anti-theoretical, but a-theoretical, a fact which may well increase its general utility. It may also be pertinent to note that the reviewer, who in some measure disagrees with the attitudes toward science and sociology suffusing this work, found little objectionable in the substantive sections.

Professor Lundberg and his two colleagues at the University of Washington have entered a thoughtful, original, carefully prepared product in the high competitive introductory textbook market.

SHELDON STRYKER
Indiana University

Essays on Sociology and Social Psychology. By KARL MANNHEIM (Edited by PAUL KECSKEMETI). New York: Oxford University Press, 1953. viii, 319 pp. $6.50.

These essays deal with social theory, the sociology of knowledge, and relations between personality and social structure. Part One begins with Mannheim's revised doctoral dissertation, "Structural Analysis of Epistemology." Little hampered by empirical referents, its interest is mainly philosophical.

Of considerable scientific importance, however, is the second essay, "Conservative Thought." Mannheim here extends the concept of "art style" to political and intellectual expressions. He then seeks to show congruences between those "styles of thought" and the socioeconomic interests of the classes and other groups which "carried" them. All this is presented against the background of the dominant social processes and structural changes in Ger-

man society after the French Revolution. Though the overwrought text forces the reader to dig for the author's concrete points, the net result is still a brilliant achievement.

The main attraction of this book for most sociologists will probably be Part Two, "German and Western Sociology." Mannheim experienced both traditions. His 1932 review article on Stuart Rice's *Methods in Social Science* is a thoughtful and still relevant evaluation of certain features of American sociology. He greatly admires its cooperative specialization and its practical bent. On the critical side Mannheim finds "a painful disproportion between the vastness of the scientific machinery employed and the value of the ultimate results" (p. 187). He refers to a fear of theories and to an overeagerness for quantitative precision. And he frankly states that his German colleagues tend to err in the opposite direction.

A companion essay describes German sociology, 1918–1933. Its chief intellectual influences, according to Mannheim, were (1) Hegel, who taught that social facts should be viewed in a theoretical framework of the whole social situation; (2) Marx, who made more realistic the Hegelian dialectic (history conceived as a resultant of opposing forces) by emphasizing the economic and class aspects of society; (3) Dilthey and Simmel, who stressed the cooperation of cultural disciplines, and who held the aim of social science to be "understanding" of the actor's meanings rather than quantitative explanation of behavioristic externals. Equally decisive for recent German sociology was its social milieu of acute conflict, which fostered in particular the first two of these influences.

By contrast, Mannheim suggests, American sociology early became preoccupied with the immediate problems of a rapidly growing and mobile society. Hence arose the "isolating empiricism" which tended to veil consideration of the social structure as a whole, especially in its dynamic and class aspects. We should probably interpret Mannheim's judgment as a relative one, made in the light of his dialectically oriented German background. American sociologists have really given considerable attention to social class, but usually in a middle-class and non-dialectic vein.

Two short papers constitute Part Three, "Sociological Psychology." The author emphasizes the falsity of the line between society and the individual. American social scientists acquainted with Giddings, Cooley, Mead and the neo-Freudians will find in this section new illustrations of a familiar principle.

The book concludes with four 1938 Oxford lectures on "Planned Society and the Problem of Personality." Mannheim is one of the few modern sociologists who courageously acknowledged the inevitability of centralized social planning, and who urged that social scientists devote more attention to the problems of a planned society. Here he restates his idea that, once the transition period is over, a planned social structure can be combined with "unplanned areas" for free individual development.

In terms of the prevailing American traditions of objectivity and quantitative empiricism, much of Mannheim's work may seem unscientific and ideological. But science has many mansions, and there is surely room for more scrutiny of the great problems of social change and conflict that dominate this revolutionary epoch. Indeed, in the light of Mannheim's sociology-of-knowledge approach, our current norms of "scientific objectivity" may themselves be viewed as an ideology of escape and conformity. With political restrictions on American scholars now being tightened, whatever conceptual tools can help us to understand our social situation are valuable.

This book, ably edited by Paul Kecskemeti, is not Mannheim's most important one. Yet it is a stimulating production by a theorist whose stature, already substantial, seems likely to grow in future estimation.

ARTHUR K. DAVIS
North Calais, Vermont

Theory and Method in the Social Sciences. By ARNOLD M. ROSE. Minneapolis: The University of Minnesota Press, 1954. xii, 351 pp. $5.00.

This book is a collection of articles and memoranda most of which have appeared in scientific journals within the last five years. With the exception of the first four chapters on which we shall comment presently, the book lacks the coherence and systematization which the title would lead one to expect. There is, however, an important compensating factor in the consistency with which clearly stated premises are applied to various subjects. On the whole the book impresses one as a genuine contribution to general theory, and puts Dr. Rose in the forefront of sociologists currently concerned with the logical and methodological foundation of their science.

The book is especially commendable for its clear and vigorous style. Whatever subject Dr. Rose develops, and he deals extensively with such important questions as the selection of problems for research, the nature of generalizations in the social sciences, and the problems of attitude measurement and public opinion, he is never didactic in his approach. The most engaging feature of his analysis is

the extensive use of illustrations from the everyday workshop of the social researcher.

There is here none of the pretension and unreality so characteristic of current attempts to develop social theory either in highly abstract terms, or through the use of mathematical models. Instead Dr. Rose deals realistically with problems that stem directly from current studies of social phenomena, and he phrases his discussion in terms familiar to every well-educated sociologist.

Undoubtedly, the special attraction of the book is the first chapter. It contains the essay which won a one thousand dollar prize when it was presented at the 1952 meeting of the American Association for the Advancement of Science. The essay sets forth a "Theory of Social Organization and Disorganization" which Dr. Rose summarizes as follows (p. 12):

"People are able to act together in an organized manner over an indefinitely long period of time because they have internalized a large number of meanings and values, commonly understood and adhered to, which permit them to make fairly accurate predictions about one another's behavior. Social disorganization—in the form of one or more of the familiar social problems—occurs when a significant portion of meanings and values are no longer sufficiently internalized to guide the behavior of a significant portion of the individuals still in physical contact with one another."

The theory is not "new" of course. Rose makes it clear that its roots are deeply imbedded in the sociological tradition. He refers in particular to the works of Durkheim, Simmel, Sumner, and Thomas. Surprisingly, he never mentions the fact that the theory has been stated by Sorokin in virtually the same frame of reference.

However, Dr. Rose's distinct contribution is his searching analysis of the implications of the theory. These are first of all stated in terms of the assumptions and general propositions which underlie the theory. Further implications are developed by a brilliant attempt to ascertain the logical derivations from the theory and to state them as hypotheses for research. Sixteen such derivations are discussed by Dr. Rose. The fruitfulness of his approach is quite apparent and strongly suggests the worthwhileness of further elaboration and extension. The possibility that this might result in an impressive contribution to systematic sociology is indicated by the content of Chapters II–IV. In these Dr. Rose applies his ideas to the elucidation of the problems of Mass Society and of the functions of Voluntary Associations in contemporary social structure. Chapter IV is a research report which essays an explanation for the minor role played by voluntary associations in French communities.

Dr. Rose's book offers a sane and balanced view on controversial issues in Social Theory and should prove particularly valuable as a basic text for seminar discussions.

THEODORE ABEL

Hunter College

Realms of Value: A Critique of Human Civilization. By RALPH BARTON PERRY. Cambridge: Harvard University Press, 1954. xii, 497 pp. $7.50.

As befits a work of its eminent author, this volume exhibits a tremendous range of scholarship and a tight system of thought. Although Dr. Perry disclaims expertness in the many fields of human endeavor which he treats, he treads with a sure foot across the fields of the natural and social sciences, of aesthetics, of education and of religion.

The general framework of his thought is soon sketched. The starting point is men's interests. Value is what is possessed by an object of an interest. More inclusive interests are always better than less inclusive ones. Since one can sympathetically include the interests of others in one's own, the "public" side of a person is more inclusive than his "private" side and is therefore to be given precedence. Morality is the endeavor to harmonize conflicting interests. This is accomplished through the principles of freedom and universality. The first of these requires that interests be harmonized for the sake of the interests concerned, and for no other reason; the second, that they be pursued with true benevolence so that each person endeavors to realize the interests of others, which he incorporates in himself. The operation of these principles produces the supreme good—harmonious happiness. One's duty is always to work for this supreme good.

Perry takes a nominalistic view of social organization. Though he admits that there are characteristics of the whole that are more than the sum of the parts, these characteristics have to do with the relationships of the parts and are not real emergents on a new level. "There are certain operations and relationships which are *exclusively internal* to a person. . . . Among these are verification, learning by experience, inference, the mediation of interest. . . . It follows that insofar as a society derives its unity from thought, from the control of action by ideas, from the relation of means to ends, and from the control of higher interests, it borrows these unifying principles from its members. They unify the interests of different persons

only when through sympathy, imagination, representation, and benevolence these are brought together in one person" (pp. 147–8).

Most of the book is taken up with a discussion of social institutions and the sciences that relate to them. These are classified into the moral, the non-moral, and the super-moral. The non-moral fall into the cognitive and the aesthetic institutions—natural science and art. Although these can be judged externally on moral grounds, the principle of freedom requires that they be judged primarily in terms of their own purposes. This is the job of the science of science and of aesthetics. The super-moral institutions are education and religion, super-moral because they involve purposes over and beyond living peacefully with other men. Though it is in the chapters on non-moral and super-moral disciplines that the author carries out his most acute analysis sociologists will be chiefly interested in the five chapters on moral institutions and their disciplines, and the treatment of sociology and history, which fall in a different category.

In what seems an odd use of terminology, Perry calls conscience a social institution. What he is referring to is moral custom or the body of moral norms. "Conscience" is used perhaps because it preserves the nominalism of his approach, though he is realistic enough to say: "An attitude which is only personal plays a highly important role in the disruption or alteration of conscience, but until it has won widespread agreement it is at most a conscience in the making" (p. 186). Because conscience is the channel by which moral judgments are applied to polity, law, and economy, it represents morality more directly than the others. Neither conscience nor its science, ethics, is to be confused with morality, however. Conscience can be criticized by ethics in terms of morality—as is exemplified in the present necessity of creating a conscience that will support international institutions.

In each of the three chapters on polity, law, and economy and their corresponding sciences, the author calls for three kinds of analysis—explanatory, normative, and technological: why do events happen thus? Are these events good? How can we influence their course? Perry would thus have social scientists make frankly normative judgments; and this seems to him legitimate because he has a clear touchstone of the morally good. The reference to the moral purpose of economy in the following paragraph is typical:

"It is one of the unhappy results of economic thinking that this ulterior purpose of economy is so often allowed to remain in the background, to be brought forward only when the system is on the defensive. Too often the economic thinker instead of surveying economy as a whole and judging its processes and instrumentalities by the moral good, puts himself in the position of adviser to a special client" (p 260).

In a brilliant chapter Perry treats democracy as a social system endeavoring through its institutions to realize morality. While seeing all the dangers and the obstacles, he points clearly to the developments in the world which he thinks necessary to make democracy international. In keeping with his social nominalism, he believes that national states cannot participate in a democratic world order unless they are internally democratic. "Nations in their composite capacity cannot agree—only persons can agree. For agreement is a form of choosing, and only persons are capable of choice" (pp. 294–5).

Perry reserves a special role for sociology and history. They are concerned not with particular institutions, but with the whole of culture or civilization, with the relationships among the parts. Although the chapter on history is full of fascinating insights and cogent criticism, we must pass over it here and concentrate on his critique of sociology.

Perry feels that sociologists have sinned in two directions. First, we "suffer from a timidity engendered by methodological scruples" (p. 356). Because we have desired to be meticulously scientific we have shrunk from analyzing the totality of man's life. We have sold our "birthright for a mess of positivism"; and our subject "dissolves into travelogues, or into psychology, anthropology, and geography, or into the several cultural social sciences" (p. 357). He wants us to attack our proper subject matter boldly and not be deterred by the bluntness of our analytical tools. His second criticism is, of course, that we refrain from normative judgments. It is at this point that Perry himself seems most open to criticism. He does not make it sufficiently clear here or elswhere that one can use norms as objective data in the course of explanatory analysis. Sociologists have done this with some success. It is true that they rarely have made normative judgments but this does not mean, as Perry sometimes implies, that they refuse to treat the value side of life. Most of his chapter on sociology is a careful and stimulating discussion of how we might fulfill what he believes to be our normative task.

Though the author's position is not one that most American sociologists are likely to accept, they can profit by studying this volume. Nothing is more valuable than to look at the familiar from a new perspective. Perry has indeed given us an outside view of ourselves,

a view based upon a remarkably clear and mature system of thought.

ROBERT C. ANGELL
University of Michigan

The Present As History: Essays and Reviews on Capitalism and Socialism. By PAUL M. SWEEZY. New York: Monthly Review Press, 1953. viii, 376 pp. $5.00.

These essays, written from 1938 to 1953, seek to view the present in terms of basic social forces developing from the past toward the future. The 34 articles, some published for the first time, are grouped under six headings: Character of Our Epoch, Imperialism, American Capitalism, German Capitalism, Thinkers and Theories, Some Problems in Political Economy. Scholarly and clearly written, the book is significant for several of the social sciences, particularly sociology and economics.

The first essay evaluates the leading ideas of the Communist Manifesto after one hundred years. Why did socialism, considered by Marx as the inevitable result of capitalism, come first in noncapitalist countries instead of in advanced capitalist nations as Marx predicted? Sweezy's answer is imperialism, a set of factors that developed mainly after Marx's time. Four chapters deal with the addition of these variables to the Marxian schema by later writers, and with the present status of imperialism. Other articles on social theorists include an illuminating review of Toynbee, a definitive critique of Burnham's *Managerial Revolution,* and incisive papers on Veblen and Keynes.

Especially relevant for sociologists is the 1951 essay on American class. This is the first original and reasonably realistic application of the Marxian schema to contemporary class phenomena, yet a recent anthology on social class does not mention it. Economists will appreciate the reprinting of Sweezy's 1939 study of interest groups in the American economy, a pioneer analysis hitherto available only in an appendix of a relatively inaccessible government document. The articles on the economic theory of Pigou, Hansen, Keynes, and Hayek are sufficiently nontechnical to interest the general reader. Three other economic essays, including one on Schumpeter, are primarily for specialists. Five chapters are brief notices of trifling books which add little to our insight.

In several respects this book is germinal. It shows that Marxism in the hands of leading thinkers is a systematically developing tradition rather than a rigid orthodoxy. This is something many conventional sociologists and Marxists need to realize. The book also suggests that academic social science will not deal adequately with social change without coming to terms with Marxism. In both sociology and economics the prevailing theoretical approaches are static and taxonomic—e.g., institutional analysis in sociology, equilibrium analysis in economics. Surely, in this age of drastic social change, this is a paradox.

Another basic issue Sweezy's book raises is the relation of social science to social policy. Marxism, of course, is not only an analytical theory but also an action program. Its purpose in viewing the present as history is to furnish a guide to future policy. Academic sociology, however, claims to be primarily non-normative. Seen against a perspective of modern science and of sociology as founded by Comte, the latter view rather than the former appears to be an aberration. Expanding the area of predictability and control of phenomena, both natural and social, has been a central premise of science. Sweezy gives no blueprints for the future, beyond calling for closer association between theory and practice.

The Present As History contains some solid analysis and some very controversial ideas. It is a challenging book.

ARTHUR K. DAVIS
University of Pennsylvania

The Mystique of Modern Monarchy: With Special Reference to the British Commonwealth. By PERCY BLACK. London: Watts and Co., 1953. xii, 92 pp. 8s. 6d.

Here is a problem ripe for functional analysis: How is it that the English monarchy, an institution of seeming irrationality in an age of nuclear fission, has persisted and, indeed, flourished luxuriantly for centuries? Surely, the persistence of this institution is no less challenging and worthy of study than the rain ceremonials of non-literate peoples. Ordinarily, the usual absence of a historical literature constrains the anthropologist to the cross-sectional analysis of tribal practices; in this case, however, no such constraints exist. There is in fact an abundance of historical documentation and monographs which one might have expected the author to draw upon systematically. Regrettably, the distinctive possibilities of the problem, and of the available materials, are treated in a cavalier manner. Many a study of prehistoric man is more adequately documented than this study of modern monarchy.

The author's research method may be described in his own words: "Whenever I observed or read about some activity involving reference to the monarchy, or about some activity of the royal family, I made a note of

it, and added it to my list." Unhappily, even this scrapbook methodology is not without its hazards, for as Dr. Black adds, "In the end there were so many items, both important and unimportant, that I was forced to make some organization among them."

The author spends much of his 92 pages suggesting the influence of the school, church, and family in transmitting the "regal sentiment," but this is done almost entirely on the basis of speculation and diffuse impression. Concerning the role of the school, he writes, "I do not know these important facts, nor does anyone else. . . ." Concerning the church's influence, Dr. Black comments, "Most of what can be said about the matter is conjecture"; he then proceeds to say it. We may concur with the author when he sighs, "Such big questions: such big voids." One is compelled to wonder what prevented Dr. Black from going out and gathering some of the data to answer a few of these questions.

At times, it appears as if Dr. Black were intent on out-Parsonsing Parsons, particularly where he speaks of the relationship between monarch and people as one of "Reciprocal Complementarity." There then follows a string of tired banalities; e.g., kingship supposedly gives the individual "purpose and belongingness. It elevates him to participate in a grand activity on a grand scale. It stimulates in him a sense of universality, a sense of belonging and unity with his fellows. For the society, it offers integration and a bulwark against disorganization." And, finally, the theoretical crown jewel, "It fosters common values." If these formulae have an air of the familiar it might be due, as the dust jacket informs us, to the fact that Dr. Black had a "spell" at Harvard.

ALVIN W. GOULDNER
University of Illinois

The Design of Social Research. By RUSSELL L. ACKOFF. Chicago: The University of Chicago Press, 1953. xi, 420 pp. $7.50.

Until recent years, most textbooks on methods in social sciences have been primarily concerned with "scientific methods" discussed on a highly "idealized" level, or with techniques for carrying out social surveys and statistical analyses on a descriptive level with little or no presentation of the basic principles or assumptions involved. In this book, Russell Ackoff has gone far toward overcoming this deficiency. According to Ackoff, "Techniques refer to the behavior and instruments used in performing research operations; for example, making observations, recording data, treating data, etc. Methods, on the other hand, refer to the behavior and instruments used in selecting and constructing research techniques. . . . In this book I have made an effort to provide a (not the) methodology of social research. That is, I have tried to raise to consciousness each type of decision that can arise in the course of research and suggest methods by which these decisions can be made and criteria by which these decisions can be evaluated" (p. vii).

The book is based on a general point of view that scientific research consists of "controlled inquiry" into problems involving choice between alternative courses of action. The choice is to be determined by the acceptance or rejection of alternative hypotheses corresponding to the alternative courses of action. The acceptance or rejection of the hypotheses is to be based on the results of experiments carried out under conditions and with procedures conforming to certain "ideal" designs or "models." This general point of view has apparently been influenced by the "pragmatic" and "experimental" philosophy of Z. W. Churchman and John Dewey; while the methods are, with few exceptions, based on the work of Abraham Wald, J. Neyman, and others on the theory of "statistical decision functions" or "inductive behavior." Dr. Ackoff's contribution is his discussion of the application of this theory to social research on a level which is understandable to persons without advanced mathematical and statistical training.

The types of research problems discussed include: the definition of variables, the selection of variables, the combination of variables using weights based on ratings by judges, the definition of relationships between variables (cause-effect, producer-product, and correlation), estimation from random samples, tests of hypotheses using random samples, the reduction of errors in surveys (nonresponse, unavailables, refusals, and response error), questionnaire and test construction, the selection and training of personnel, and the determination of cost in time or money. The treatment of principles and methods related to these problems is most adequate in relation to tests of hypotheses. Here Dr. Ackoff not only presents underlying principles such as the theory of Neyman and Pearson, but, also, specific statistical methods together with their underlying assumptions. One can criticize this treatment only in minor details, such as on its lack of clarity and specificity in defining the conditions of "randomization," and the absence of a generalized application of "chi-squared" tests of hypotheses.

The discussion of estimation problems is somewhat above the level of most previous texts, but is still not as clear or rigorous as one

should expect in this text. The basic theories of estimation such as "confidence interval estimation," "unbiased point estimation," and "maximum likelihood estimation" are referred to but not systematically presented as a basis for developing and evaluating techniques of sampling and estimation. Also, although varieties of sampling methods are discussed, the majority assume "simple random sampling," and the term "randomness' is used without clear reference to possible restrictions on simple random sampling that might be used.

Although Dr. Ackoff has made a notable contribution to the discussion of research methodology, there remain a number of problems for which the treatment is disappointing and inadequate. In part this is due to the lack of adequate principles from which to draw, but in a number of cases he has not made use of the available principles, for example, on the basic definitions and rules in the "theory of probability." The discussion of questionnaire and test construction does not make use of the statistical theories developed by people like Thurstone, Guilford, Guttman, Coombs, and Stevens. The discussion of survey errors does not include statistical principles and methods of "test-retest" reliability, such as those developed by Louis Guttman and others, and the section on relationships between variables does not include a discussion of the statistical theory of prediction developed by Ezekiel, Louis Guttman, and others.

A number of points of inconsistency or lack of clarity occur. For example, in pages 34–35, the term "probability of production" is used to express a subjective judgment, whereas it is previously defined to correspond to a "statistical frequency." "Estimation" is discussed on pages 39–40 in a manner that confuses two meanings: one, of defining one variable as a mathematical function of another, the other, of determining unknown parameters from randomly sampled values. The definitions of simple random sampling (p. 92), of probability (pp. 144–145), and of degrees of freedom (p. 182) are particularly lacking in clarity and precision.

It is also unfortunate that the author has included some contributions of his own which would be more appropriate in a book on newly developed and specialized techniques than one on basic principles. In particular, these include his techniques for ranking and weighting objectives (pp. 24–27, and 375–376), and for the construction of tests by weighting items according to ratings by judges (pp. 305–324).

In spite of such limitations, which are perhaps inevitable in any work that attempts to depart from the beaten trail, this book has made a notable contribution to bridging the gap between recently developed theories in mathematical-statistics and the numerous techniques conventionally used in social research without adequate theoretical or experimental evaluation.

JULIUS A. JAHN

Pullman, Washington

Standard of Living: The Concept and Its Place in Economics. By HUGO E. PIPPING. Helsinfors: Societas Scientiarum Fennica, Commentations Humanarum Litterarum, Tom XVIII, No. 4, 1953. 280 pp. 1,400 Mk.

This study is on the borderline between economics and sociology. The author looks hopefully to "a sort of synthesis" between them and regards his work as a step in that direction (p. 6). This is his thesis: "In conclusion, a sensible goal of economic activity would be *to raise standards in all groups without injuring any of them absolutely and rather to give the lower strata a chance of improving, supposing that the use of resources becomes thereby more functional*" (p. 253; his italics).

In thus making standard of living the central concept of economics, Pipping adversely criticizes all hedonic, marginal utility, subjective wants, desires, needs, satisfactions, and individualistic theories of economics. He finds that objective data regarding the consumption habits of groups, classes, and other social structures produce and sustain the standards which govern economic activities. He thinks the distinction between "standards" and "levels" of living is confusing. He prefers "ideal" and "minimum" standards but holds that all standards are relative, not absolute; even minimum health (nutrition) standards are different for different occupations. True standards are found by intensive empirical study of objective behavior of people in clearly defined social structures.

He designates four substandards of living: consumption, work and leisure, family, and saving, and discusses them at length. These are measurable to a considerable degree by finding the proportions of money spent for each. He then discusses how standards change.

This is a good treatise for sociologists though most of them do not need it as much as those economists who think sociology, anthropology, and social psychology have little to contribute to economic theory.

Many sociologists, and other social scientists, still need to learn from Pipping how to deal with their own "imponderabilia." It seems clear to Pipping (and to me) that all we can know about "subjective motives" is inferential and that if such inferences are to have any scientific value, they must be based upon sense-observable and measurable data processed by

the tested methods of natural science. Crucial to this is the time factor and clear definitions of the various types of social structures with which we deal. We certainly must learn not to use "group" as a synonym for "category," as Pipping and many sociologists often do.

In general, it is a carefully thought out and well written book. It may even be a landmark in the long hard struggle of the social "sciences" to become natural sciences.

This short review cannot possibly do justice to the book and hence is more or less pointless unless it may by chance influence some to read the volume who otherwise might not.

READ BAIN
Miami University, Oxford, Ohio

Rehabilitation of the Older Worker. Edited by WILMA DONAHUE, JAMES RAE, JR., and ROGER B. BERRY. Ann Arbor: University of Michigan Press, 1953. 200 pp. $3.25.

Annually, since 1948, the University of Michigan has sponsored a conference on problems of aging. The fourth of these conferences in July 1951 dealt with "Rehabilitation of the Handicapped Worker Over Forty." This volume records, and in some instances summarizes, the proceedings of the conference.

Under the theme that "all are needed," the purpose was to promote a more general appreciation of the number of handicapped older people in the population who can be returned to partial or even complete self-support and self-care. Rehabilitation, it was claimed, is possible for many of the aged who are now institutionalized or who are otherwise leading dependent existences in the community. At the same time the conference was designed to give more attention to the most effective means and techniques for accomplishing the goals of rehabilitation and to show the large savings in private and public funds that can be achieved through rehabilitation.

As is usually the case in a coverage of conference proceedings, the formal papers are featured in the book. John Thurston discussed national interest and the role of the government in rehabilitation and aging; Dr. Howard Rusk defined the magnitude of problems of rehabilitation and enumerated the steps necessary for the rehabilitation of the "total person"; Dr. Frank Krusen reviewed the history of physical medicine and the development of the rehabilitation movement in recent decades; lastly, Dr. Lionel Cosin described a program of geriatric rehabilitation in England.

In addition there is summarized the four "conference board hearings" of the conference. A conference board was composed of seven persons who represent groups concerned about rehabilitation of older workers, but who are not personally engaged in that area of professional work. The topics covered in these hearings were medical, psychosocial, and economic aspects of rehabilitation; employment and placement; and rehabilitation services and programs.

Rehabilitation of the Older Worker is more likely to be of interest and value to the geriatrician and social worker than to the sociologist. On the other hand, the volumes covering the first three Michigan conferences—*Living Through the Older Years, Planning the Older Years,* and *Growing in the Older Years*—are extremely valuable not only to gerontologists but to other social scientists as well.

MILTON L. BARRON
The City College of New York

The Philosophy of Social Work. By HERBERT BISNO. Washington: Public Affairs Press, 1952. 140 pp. $3.25.

Every practitioner, whether he be physician, pedagogue, counsellor or social worker, applies standardized techniques to his problems in order to produce preconceived changes in them. This implies goals; specific goals for individual problems, broad goals for the overall problems which are the concern of the practice. But goals, in turn, derive from and are supported by social values prescribing what is desirable and undesirable. In this sense of professional practice represents the harnessing of scientific knowledge in the service of a social philosophy. It would seem therefore that all practice disciplines might justifiably divert into clarifying their philosophical underpinnings some of the effort now devoted to improving their techniques.

The volume under review is an attempt to systematize the philosophy upon which social work practice rests. The work carries the characteristic crudity of all initial essays into systematization, but it deserves commendation precisely because it is a good first venture that is long overdue. The author has undertaken the painstaking task of explicating the philosophy of social work from the written and oral statements of social workers. He emerges with twenty-nine philosophical principles arranged into four broad classes, which may be very briefly summarized as follows.

I. *The Nature of the Individual.* The individual, by the very fact of his existence, is of worth and has the right of access to life's opportunities; individual suffering is therefore undesirable and should be prevented. Man is originally amoral and asocial, acquiring a social nature through interaction with the social environment, so that human nature is alterable.

Individual actions are not entirely of rational origin, but are also irrationally motivated. II. *The Nature of the Group Bond.* The major responsibility for the welfare of all its members, irrespective of their group affiliations, rests with the community, and the doctrine of laissez-faire is to be rejected. The function of the state is more than social control and includes providing actively for the common welfare. Political freedom and economic security are not mutually exclusive and opposing states, but are companion conditions. III. *Social Maladjustment and Change.* There are serious maladjustments in our society making imperative social planning, which is possible without resort to dictatorship. An evolutionary type of reform is desirable and, because of the flexibility of our institutions, is feasible. IV. *The Method of Social Work.* Democracy is an indispensable condition for social improvement. Human behavior can be more effectively modified through the development of conditions which generate in individuals the inner urge for change; this rejects the technique of coercion. A dualistic approach toward social problems is possible, which combines the individual-by-individual helping process with the reform of social conditions.

The author develops the logical implications of these principles, demonstrating their manifestations in social work policies and practices. He then evaluates the principles against the dominant values of American society as the latter have been described by social scientists. His conclusion is that the philosophy of social work conflicts with significant aspects of our culture, which accounts for the failure of social workers to secure support from influential segments of American society.

It seems to us that, in seeking to demonstrate the conflict between social work philosophy and the culture, the author has forced his case. He imputes to both social work and the American culture monolithic and internally consistent characteristics, arrayed against each other, which do not square with reality. After all, social work is a uniquely Anglo-American phenomenon, indigenous to our culture, and the philosophy upon which it rests is native to us. Admittedly there are strong currents opposing social welfare programs; nevertheless these programs are expanding. The word that would more accurately characterize our society's attitude vis-à-vis social work is not *opposition,* but *ambivalence.*

Evaluation of this work could be easier if the author furnished the reader his definition of *philosophy.* Many of the propositions he formulates lack the ethical and epistemological character of philosophy. They are essentially empirical generalizations produced by social science; because they are about human beings does not elevate them to a philosophical level. More efficient organization would have eliminated overlapping and duplication of materials in the volume, and thereby reduced the number of principles. The book's title conveys a definitiveness which its contents lack. We wonder whether the work might not have been more appropriately entitled *My Philosophy of Social Work.*

The shortcomings mentioned above should not be allowed to overshadow the virtues of this volume, which represents an encouraging start in a difficult area.

ERNEST GREENWOOD
University of California, Berkeley

Mid-Century Crime in our Culture: Personality and Crime in the Cultural Pattern of American States and Cities. By AUSTIN L. PORTERFIELD and ROBERT H. TALBERT. Fort Worth: Leo Potishman Foundation, 1954. 113 pp. $2.25.

This volume expounds the idea that geographical differences in crime are a consequence of differences in social structure and culture pattern. Its contents, with minor qualifications, have appeared piecemeal in sociology journals during the last five years and will therefore be familiar to readers of these magazines. For purposes of convenience, the various writings are grouped under three headings—theory, data, and action.

Although their discussion of theory (Part I) deals mainly with the sociological approach to crime, the authors lean toward, if not espouse, an eclectic position. They seem equally receptive to the ideas of Healy and Sutherland. The data on crime (Part II) consists of figures published in the annual reports of the Federal Bureau of Investigation and information derived from local records in Fort Worth, Texas. Manipulation of these statistics, along with selected demographic data, produces the characteristic result that crime, as gauged by official records, is inversely related to the socio-economic level of the population. The plea for action (Part III), which does not appear to issue from what has preceded, culminates in the recommendation that American communities make provisions "to keep normal people normal, to help those who are in danger zones, and to treat its problem cases . . ." (p. 103). Provisions suggested by the authors, like the goals themselves, are quite general, as, for example, "adequate religious training for every child," "clinical facilities for behavior problems," and "a closer working liaison between schools and employers" (p. 104).

Most ecological data have their limitations and an elucidation of these is usually in order as a check on undue generalizing. In their report, Porterfield and Talbert nowhere state that crime rates by states, as quoted in *Uniform Crime Reports,* refer only to urban populations, and overlook the possibility that rates based on rural or total populations might display a significantly different geographical configuration. Secondly, populations arranged according to political units such as states, or more natural areas such as census tracts, are certain to diverge on many factors which affect the incidence of crime. The authors note that such differences constitute a methodological advantage to the extent that their impact on crime can be accurately assessed, but fail to note that they may create a serious problem if they are impossible to control or go unnoticed. Thirdly, no consideration is accorded the principle that group correlations are restricted to aggregates and cannot logically be extended to individuals. This limitation, unless made explicit, may cause the unwary to conclude, for example, that, since the crime rate increases as secularization declines, the most sacred people are especially crime prone.

Of considerable theoretical importance, although tangential to the central theme of mid-century crime, is the datum that suicide is relatively low in economically depressed areas. This does not square with the inverse connection between the suicide trend and the business cycle as it has been observed in the United States during the last fifty years. It is to be hoped that the authors, who have made a speciality of studying homicide and suicide in the United States, will give their attention next to this contradiction which emerges in changing from a geographical to a chronological point of view.

KARL SCHUESSLER

Indiana University

The Juvenile in Delinquent Society. By MILTON L. BARRON. New York: Alfred A. Knopf Inc., 1954. xix, 349, vii pp. $5.00.

This new text by Professor Barron of The City College has some things to commend it. It is clearly and interestingly written; it is fairly comprehensive; and it has a frame of reference as indicated by the title. I am afraid, however, that in trying to avoid "sins of omission" the author has fallen into "sins of commission."

Part One includes a discussion of the definition of the problem of delinquency, proceeds with the available statistical measures of the amount and kind of delinquency, and then shows the fluctuations in delinquency according to the business cycle and in times of war and peace.

Part Two, called the Etiology of Delinquency, after presenting a chapter on the philosophy underlying causation and another setting forth the various approaches to delinquency causation, discusses what is known about each of the approaches in seven separate chapters under the titles: The Biology and Psychology of Juvenile Delinquents; the Family and Juvenile Delinquency; Peer Groups and Juvenile Delinquents; Institutional Omissions and Commissions, The Delinquent Culture of American Society; and Ecological and Chronological Factors.

Part Three deals in four chapters with the way in which delinquents are handled from detection to parole. A final chapter reviews various control and prevention programs.

A series of eight prints from the engravings by William Hogarth, an English artist of the eighteenth century, are used throughout the etiology section. These heavily satiric drawings of the society of the time are supposed, according to the dust cover, "to amplify" the theme of a delinquent society.

Having tried to give a brief resumé of what the volume contains, this reviewer would like to point out what in his opinion are the "sins of commission."

First, to the student beginning the study of delinquency or criminology this book can only be a rather confusing approach. The author's general treatment of etiology consists of reviewing a wide range of studies dealing with each subject he discusses. The results of the studies are often given without comment as to whether or not they are based on sound research procedures, and are cited or quoted out of chronological sequence. A 1947 citation may be followed by one from 1903. The real difficulty, however, stems from the fact that after having cited these studies Barron tends to dismiss them as without importance because some delinquents fall in the opposite category from that which he is at the moment discussing. Of course, this proves that none of the specific items discussed causes juvenile delinquency, but this seems only to attack a strawman, for no careful student of delinquency today would maintain that any single factor is the cause of delinquency. Furthermore, it would seem that the author's own argument and discussion are ignored in the chapter "The Juvenile in Delinquent Society," for in this place the distinct impression is created that certain aspects of society assure delinquency, with no attempt to explain why only a very small proportion of youth

becomes delinquent. The real difficulty, it would seem, is that Professor Barron fails to attempt to bring these many factors together in some integrated whole. Throughout this section sweeping generalizations are made.

Second, this reviewer cannot help but wonder what the Hogarth prints contribute to an understanding of delinquency. Although these prints are "indubitably unique and dramatic" they certainly do not illustrate present attitudes or even conditions.

Finally, in Part Three there is much to confuse the student. Parole, probation, foster-home care, and other similar concepts are discussed as alternatives to treatment. These are not alternative but part of the total treatment picture.

It is too bad, after the tremendous amount of work which must have gone into this book, that the total product could not have been more satisfactory.

H. Ashley Weeks
New York University

The Juvenile Offender: Perspective and Readings. By Clyde B. Vedder. New York: Doubleday and Co., Inc., 1954. xii, 510 pp. $6.00.

This book is a collection of readings drawn mainly from professional journals in the fields of corrections, criminal law, and casework. For the teacher of courses in juvenile delinquency it provides a body of materials produced by probation and parole workers, case-workers, legal experts, juvenile court judges, and others engaged day to day in dealing with young offenders. Introductory explication, briefly but soundly developed, prefaces each section and is intended to make the volume useful as a textbook in juvenile delinquency as well.

The author's intention of bringing to the student an "authoritative" interpretation of the problem of the juvenile offender raises a complex problem. On the one hand only those who are continuously confronted with the intractable and irritating hostility of the delinquent and who must use the established institutional means for "adjusting" the deviant have access to the empirical materials for forging new and more useful perspectives on the problem. On the other hand workers in this field, as in most other professions, tend to accept uncritically the ideology of the profession and to respond to the challenge of their task by reiterating the shibboleths of the correctional field. As a result, articles of the type making up most of this collection can be both of little use and of exceptional value in elucidating the problem of the juvenile offender. Superficially considered, these writings do little more than restate the dilemma of corrections in the customary terms of individual versus group welfare. At a more profound level, however, this collection of articles provides a body of basic data with respect to the assumptions, the attitudes, and the institutionally determined compulsions of those who act for society in dealing with the delinquent child. For those teachers who wish to deal with problems of this nature Mr. Vedder's book should be of inestimable value.

Apart from these more general considerations, a number of the articles included make a genuine contribution to knowledge about delinquency, or state eloquently and sharply the conflicts currently agitating the field of juvenile correction. The former are well represented by Dumpson's report of an experiment conducted in New York City to discover effective methods of breaching the barriers which have traditionally made "hard core" groups of street corner delinquents inaccessible to correctional workers. Representing the latter is an article by Judge Perkins dealing with apparently irreconcilable contradictions between established principles of criminal justice in our culture and principles underlying the widely adopted Youth Correction Authority acts.

Solomon Kobrin
Illinois Institute for Juvenile Research

Analysis of Social Problems: Supplemented by Case Materials and Selected Readings. By Morris G. Caldwell and Laurence Foster. Harrisburg, Pennsylvania: The Stackpole Company, 1954. xv, 715 pp. $6.50.

This text has an extremely eclectic approach to social problems although one would scarcely expect otherwise with two editors and sixteen different chapter authors.

To his credit, Caldwell rejects the social problems approach, i.e., treatment of all types of problems in a purely descriptive manner without a basic conceptual scheme and, in addition, rejects the psychological, psychiatric and other single factor explanations of social disorganization. Caldwell contends that one unique feature of this volume is the development of a functional frame of reference, i.e., social problems are functions of basic social structures and social processes. Social structures include cultural systems, social organization, social institutions, class and caste systems, relational systems, social groups, social situations and values. Processess include social interaction, social participation, social disorganization and social change. The uniqueness

of this all-encompassing frame of reference is doubtful. Caldwell's procedural steps in terms of his conceptual scheme turn out to be none other than the four basic steps of the scientific method. Chapter authors seem neither entirely aware of Caldwell's frame of reference nor of his procedural steps.

There is also a tendency to include material uncritically, even from popular sources. The following selection from a supplementary reading, *What We Should Know About Marihuana* by Bertha Rechel Palmer, the sole reference to drug addiction (pp. 364–5), is a case in point. "Under the influence of marihuana, the mind seems to lose the power of directing and controlling its thoughts—all sense of values seems to disappear. . . . The drug has a corroding effect on the body and mind, weakening the entire physical system and often leading to insanity, hence the name 'weed of insanity'. . . . Smoking marihuana creates a mental habit which is dominant, controlling and exciting to cruel and revolting crimes. . . . One dose for the time being kills the best in the user, and it may cause him to kill others, and even kill himself—hence the name, 'The Killer Drug.'" This perpetuation of popular misconceptions about marihuana stands in contrast to the experimental findings of Allentuck, Bowman, Halpern and others as well as the conclusions of more critical textbook writers.

One or more chapters are devoted to each of the following subjects: population problems, minority group relations, modern technology, industrial relations, economic insecurity, physical health and disease, socialization, personality disorganization, mental health, marriage and the family, parent-child relations, the aged, educational problems, political and governmental problems, crime and delinquency, community organization, social class, war, international organization, social planning and social action. Readers may be surprised at the appearance of two chapters on socialization midway in the text.

LYLE W. SHANNON
University of Wisconsin

Turrialba: Social Systems and the Introduction of Change. Edited and directed by CHARLES P. LOOMIS, JULIO O. MORALES, ROY A. CLIFFORD, and OLEN E. LEONARD. Glencoe, Illinois: The Free Press, 1953. viii, 288 pp. $3.50.

This monograph represents a cooperative endeavor by the Inter-American Institute of Agricultural Sciences and Michigan State College. The study was made at Turrialba, Costa Rica, where the Institute is located. Turrialba is a town containing about 6,000 inhabitants and an outlying trade area with 13,500 additional inhabitants living in 32 villages and in the open country. The data were collected in the following manner:

> A carefully supervised sample census project containing a considerable amount of level living data with full coverage had been administered in the Turrialba district, and census croquises showing house locations were available. In 1948 a random sample, stratified by villages for the area, which included about 700 households, had been drawn and used as a basis of measuring levels and standards of living for both rural and urban groups. A ⅛ sample was drawn for the town of Turrialba with its 6,359 inhabitants and, depending upon the density of population and closeness to Turrialba, a ¼ to ½ sample of the 32 villages and open country households of the area was drawn.

The work falls into the realm of applied social science and is aimed largely at developing a strategy of change. The various chapters supposedly represent the social systems considered most important in the strategy of change. A list of 23 hypotheses relative to the main topic are presented in the concluding chapter.

Sixteen authors representing several different social science disciplines participated in the study in one way or another. The work contains 14 chapters, most of them by a different set of from one to five authors. The scope of the work can best be indicated by the various chapter headings. After an introductory chapter by Loomis and Morales, chapters follow on "The Setting of the Study," "Social Status and Communication," "Informal Social Systems," "Economic Systems: Large and Small Land Holdings," "The Ecological Basis of Social Systems in Turrialba," "Demographic Characteristics of the Population," "Health Systems," "Religious Systems," "Educational Systems," "Agricultural Extension Systems," "Political Systems," "Levels of Living on Haciendas and Small Farms," "Study of the Strategy of Change on Large Estates and Small Farms in Latin America."

The work will be very useful for persons contemplating research projects in Latin America and will be especially valuable for extension workers in any country. On the negative side, the work, as might be anticipated, appears to suffer somewhat from lack of coordination due probably to the large number of authors involved. There is considerable repetition in the various chapters. The authors make use of the "Social systems" concept as defined in the rural sociology text by Loomis and Beagle, *Rural Social Systems,* yet at times the term

"system" or "systems" is used rather loosely and is sprinkled so frequently into the text that it seems to do violence to the technical concept (see, for example, Chapter X). Nevertheless, the work represents an important contribution to the understanding of Latin American society and deserves wide reading.

N. L. WHETTEN

The University of Connecticut

So This Is College. By PAUL H. LANDIS. New York: McGraw-Hill Book Company, Inc., 1954. ix, 205 pp. $3.00.

Strictly speaking, this is not a sociological work; rather it belongs in the tradition of "functional" books designed to assist young people better to find their way in the real (or assumed) complications of a strange world. Specifically, this book contains advice about adjustment to college. Any evaluation of necessity will reveal more of the reviewer than of the writer because, there being no really objective standards for a work of this kind the reviewer can merely compare his with the author's subjective appraisal of the need for a book of this sort and the extent to which this particular book fulfills that need.

American college students are a heterogeneous lot. They range from abysmal naiveté to sophistication often exceeding many of their teachers. Some are provincial in the extreme; others startlingly cosmopolitan. To write an "advice book" to meet the needs of all would seem to be impossible and, for that matter, quite unnecessary for a substantial percentage. It seems that this book would be most useful to potential or beginning college students with several of the following characteristics—from the farm or very small town, from the lower middle class, from conservative religious antecedents, with substandard parents and peer experience, and with a high quotient of neurosis. Students with the opposite social characteristics will find the book either unnecessary or moderately humorous. Placed in the proper hands, then a set of chapters on "Getting Wise to Ourselves," "Flying Away to Dreamland," "Conflicts, Glands and Guilt," and "The Destiny Within Me" might be most appropriate, while for many other mature, healthy young people would seem either a waste of time or an amusing reminiscence.

The book's readability is facilitated by a large number of paragraphs excerpted from students' autobiographies, in which their own problems of adjustment to college (or merely to growing up) are treated with candor and, rarely, with insight.

Despite the tendency of many academicians to dislike and often ignore the fact, it still remains that one of the functions of a modern college and university is custodial rather than educational. In its role of parent surrogate the institution of higher learning needs, perforce, to perform for some students the kinds of service for which this book is intended. In the hands of the needful student—but clearly not of all students—the book may well turn out to be exceedingly useful in a number of ways.

JOHN F. CUBER

Ohio State University

BOOK NOTES

Detroit as the People See It: A Survey of Attitudes in an Industrial City. By ARTHUR KORNHAUSER. Detroit: Wayne University Press, 1952. xi, 221 pp. $6.00.

This is a report of the polling study of the attitudes of a representative cross-section sample of 593 Detroiters (men and women) toward their city. In this reviewer's opinion it deserves a more extensive discussion than the space assigned in a Book Note. Little can be said about the actual findings and nothing can be written about the methodology, although the sampling procedure and questionnaire (in the appendix) would be of considerable interest to many.

Tables and selected samples of comments from the respondents give answers to questions such as: How favorable do Detroiters feel toward their city? (A majority are favorable.) What do they like and dislike about the city? (Twenty-one to thirteen per cent like Detroit as a place to work, the physical characteristics, personal considerations such as home and friends, and opportunities for recreation and sport. Twenty-two to ten per cent dislike the physical characteristics, the kind of people who live there, the transportation system, and the traffic and parking problem.) How do they appraise certain selected activities? (Housing and Negro-White relations headed the list.) What do they feel can be done to improve the city? (Few have concrete suggestions.)

The answers to these questions are presented as facts with no attempt to evaluate them, as the author feels that it is not the function of this kind of research "to pass judgment on whether people should think and feel as they do." Rather the results, as the book states in its closing sentence, provide "starting points and guide posts for all who are interested in pushing forward programs to make Detroit an even better city than it now is." This constitutes a challenge to the government and various civic organizations. The Detroit Board of Commerce is to be congratulated for making the study possible through a grant to Wayne University.—H. ASHLEY WEEKS

American Geography: Inventory and Prospect. Edited by PRESTON E. JAMES and CLARENCE F. JONES. Syracuse: Syracuse University Press, 1954. xii, 590 pp. $6.00.

The fiftieth anniversary of the Association of American Geographers is marked by the publication of this volume on modern American geography. As indicated by the title, this volume is a stock-taking of the present status and future prospects of the field. It constitutes ". . . a progress report on the objectives and procedures of geographic research as of January, 1954 . . ." and contains considered suggestions for directions of future geographic research.

The twenty-six chapters of this scholarly and thoughtful volume were written by about the same number of geographers, with the active assistance of as many more of their colleagues. Unlike so many books in which chapters are written by different persons, the contents of this volume are well articulated considering the scope and variety of the specific subject matters. A conscious attempt must have been made to integrate the various chapters around a few basic concepts.

Each of the chapters is devoted to a particular specialization within the general field of geography. These range from "The Regional Concept and the Regional Method" and "Historical Geography" to "The Geographic Study of Soils" and "Physiological Climatology." While much of the material is not of interest to the sociologist qua sociologist, e.g., agricultural geography, geomorphology, etc., the chapters on urban geography, the geographic study of population, political geography, economic geography, and others contain excellent summaries of the contributions of geographers to topics of teaching and research interest to sociologists and other social scientists.

For geographers this volume undoubtedly will be the beginning of new and rewarding lines of research; for other social scientists it is a useful reference to the products of two generations of geographic research.—WENDELL BELL

The Impact of Strikes: Their Social and Economic Costs. By NEIL W. CHAMBERLAIN and JANE METZGER SCHILLING. New York: Harper and Brothers, 1954. viii, 257 pp. $4.00.

This volume is an attempt to test a quantitative device developed to measure the economic and social costs of strikes. The strike rating procedure used here was first developed in another study by the senior author. The procedure is described in several chapters and the rest of the volume is devoted to testing this procedure by analyzing seventeen strikes in three industries in coal, railroads, and steel. The categories used in determining impact are based on the question of how the strike affects the total public and its component parts, such as consumers, producers, suppliers, and users. A number of limitations are cited by the authors in the use of their procedure, one being that the rating given to a particular strike only applies to the day of the rating and not to the total effect of a strike. The data to secure a day-by-day rating are not available. The purpose of this study was to determine the impact of strikes on the public so as to provide information for policy decisions by the government.

In spite of the fact that the authors recognize that strikes are as complex as our economy and their impact often cannot be measured by quantitative devices in advance of the strike, they advocate that some type of direction should be given to the government in the development of legislation and administrative decisions. Thus, the author's strike rating procedure is offered as an improvement over existing ways of measuring the impact of a strike upon the public and its component parts. A procedure of this type doesn't attempt to measure the many intangibles that must be examined in any discussion of the impact of a strike. In fact, the total impact of a strike cannot be measured by dollars and cents or by the measurement of the extent of inconvenience to the public alone, but must also take into account many intangibles operating in a strike situation, such as the prestige of the union with its members and its show of strength to the company; long run impact

of a strike upon future bargaining relationships; effect upon future concessions gained by the union from management, etc.—JACK LONDON

Race, Jobs and Politics: The Story of FEPC. By LOUIS RUCHAMES. New York: Columbia University Press, 1953. x, 255 pp. $3.75.

This book presents a comprehensive account of the effort to achieve fair employment practices through governmental intervention. While one chapter each is devoted to state and municipal fair employment practice legislation, FEPC legislation and other social problems (segregation, full employment, etc.), and the history of the movement to achieve permanent fair employment practice legislation on a national level, the major emphasis has been placed upon a study of the origins, history, and achievements of the President's Committee on Fair Employment Practice which was created by the late President Roosevelt in 1941. The President's Committee is emphasized "not only because it is the parent of all later fair employment practice commissions but because in its history and achievements one finds the answers to questions relating to the practicability and effectiveness of federal fair employment practice legislation."

Three "answers" for the future noted by the author are: (1) in the elimination of discrimination in employment, education and legislation serve to bolster and sustain one another, for legislation provides a frame of reference within which the necessary educational processes can be accelerated; (2) opposition and strike action against the employment of minority groups are usually the product of agitation by a small nucleus of workers and once this nucleus is isolated and neutralized the vast majority of workers tend to adjust themselves to the new situation without difficulty; and (3) an examination of the variations in resistance to employment and upgrading of Negroes in the transportation systems of Philadelphia, Los Angeles and Chicago suggests that the differences were more closely related to the type of labor-management relations prevailing within each situation than to differences in intensity of "race prejudice."

The book is factual and well-documented for the area it covers, although this reviewer wishes that more attention might have been given to the state and municipal bodies even at the expense of some of the historical materials. The book is well-written and should prove of interest both to general readers as well as to social scientists.—PRESTON VALIEN

Central European Democracy and its Background: Economic and Political Group Organization. By RUDOLPH SCHLESINGER. London: Routledge and Kegan Paul Limited (Grove Press, distributor), 1953. xiv, 402 pp. $5.00.

The title is misleading: it is not a study of democratic institutions in Central Europe, but an interpretation of the role of the labor movement in the trend of events that culminated in the rise of fascistic regimes in Germany and Austria.

The treatment of the trade union and socialist movements is thorough and competent. The author has an admirable knowledge of facts and keen sociological insight. It is one of the merits of the book that it is not confined to one country but deals with the movements in Germany and Austria, also Czechoslovakia, as closely related phenomena.

The author is quite frank about the subjectivity of his interpretation, though in the course of the treatise his opinions seem to become more and more dogmatic.

Unfortunately, much of the potential effect of the book is lost because of the tortuous and often quite obscure style.—RUDOLF HEBERLE

Ting Hsien: A North China Rural Community. By SIDNEY D. GAMBLE. New York: Institute of Pacific Relations, 1954. xxv, 472 pp. $6.50.

Since the impact of modern science upon China began to be felt more than a generation ago, the undertaking of community surveys and studies has become more and more popular in that country. Among them, the work done at Ting Hsien, a county located in the North China Plain, in cooperation with the Mass Education Movement is by far the most comprehensive and substantial. This reviewer visited Ting Hsien in 1933 and is quite familiar with the field studies directed by Franklin C. H. Lee and with the resulting three-volume, Chinese publications whose titles are mentioned in the Foreword. It is a pity that English readers should have waited some twenty years to see a translated counterpart of those Chinese volumes.

Ting Hsien contains, to a large extent, materials of a census, but it also includes discussions of customs, folklore, and historical and geographical background of that county. Of great value are the 22 charts and 116 statistical tables which range in content from figures on

population, agriculture, cottage handicraft, family budgets, marketing, to those on government, taxes and education—all of which are based on primary sources. The inclusion of folk dramas, complete and summarized, may be justified on the ground that to those who do not have first-hand knowledge of a Chinese rural community, these folksongs express intimately the sentiments and values of the country people.

A critic may object that this book was not done in the light of a sociological perspective, and, as a result, provides primarily numerical, descriptive accounts on many divergent subjects, without a thesis. However, both Mr. Lee, the field director, and Mr. Gamble, the present author, would probably argue that they are interested only in the presentation of concrete data in an objective and precise manner, leaving to others the tasks of generalization and conceptualization.—SHU-CHING LEE

The Sociology of Child Development (Revised edition). By JAMES H. S. BOSSARD. New York: Harper and Brothers, 1954. xi, 788 pp. $6.00.

The first edition of *The Sociology of Child Development* was reviewed in this journal by Ernest B. Harper (August, 1948, p. 499) and the present reviewer finds himself in essential agreement with this earlier critique. The book is a large one (30 chapters and nearly 800 pages) and covers considerable ground, some of which was explored for the first time. Original contributions are particularly prevalent in Part III, "Facets of Family Life"; here the author reports his pioneer research in such areas as: table talk, family modes of expression, the bilingual child, the role of the guest, domestic servants, parental occupations, and family rituals—all as related to child development. In addition, treatment is given to nearly every topic traditionally studied by those who work with children. The approach is sociological, with major emphasis given to situational factors which condition the growth of the child. But psychologists, home economists, educators, social workers, and family life specialists, as well as sociologists proper, should find this book helpful.

Revision has not been extensive. Two new chapters dealing with parents' occupations and family rituals have been added. The chapter called "War and Child Development" has been dropped. Statistics have been brought up to date, a few sentences have been reworded or regrouped, and some of the references have been changed. But there hasn't been much of this, and the reviewer is of the opinion that some of it has been hurried and superficial. As an example, there is a substituted footnote reference to one of the reviewer's studies (p. 87) which seems inappropriate. It is given in support of the premise that every baby is a "bundle of joy," when actually there is nothing in the work cited that would justify such a point of view.

Nevertheless, this book's strong points far outweigh its weak ones and it is to be recommended.—HAROLD T. CHRISTENSEN

Out of Wedlock: A Study of the Problems of the Unmarried Mother and Her Child. By LEONTINE YOUNG. New York: McGraw-Hill Book Company, Inc., 1954. 261 pp. $4.00.

This is a highly equivocal book and it is difficult to state precisely any definite findings. Miss Young sees in (or into) her cases women who conceive illegitimately, bear to parturition, and symbolically react to their child because they want to. They want to because of hostilities and other neurotic symptoms directed mostly against their mother, in a small number of cases against their father or father-substitute, and in a few cases because of general hostility. This neurotic, fatalistic compulsion arises in the pre-œdipal fixations of the phallic stage and is related to mother-dominance, father-dominance, the wish for a penis, masochistic self-punishment and so on. The illegitimate's father is also neurotic and compulsively acting out hostilities toward a dominant mother or surrogate. "In cases where the unmarried father is known, the corollary between his problems and those of the girl is even more apparent" (p. 137).

Professor Young recognizes and decries the use of law as a bludgeon, deleterious public attitudes, inadequacies in public welfare policies, and the overlapping, competing and sometimes exploitive social agencies. She recommends that caseworkers employ current canons of good case-work in dealing with these cases. The ultimate, but Utopian solution to social problems lies in producing happy children, she says.

The rare use of statistics is polemic and unsound. The study is naive and unsophisticated, without critical analysis, employing a haphazard sort of methodology. The result may be good journalism, but it is poor science.—FRANK F. MILES

The Traffic in Narcotics. By H. J. ANSLINGER and WILLIAM F. TOMPKINS. New York: Funk and Wagnalls Co., 1953. xi, 354 pp. $4.50.

The United States Commissioner of Narcotics and the United States Attorney for the District of New Jersey have pooled their experience and efforts to produce this semi-official statement on the nature of the narcotics problem and appropriate methods of dealing with it. It cannot be recommended as a source of information on the subject, for fact and fancy are inextricably intertwined throughout the exposition, and many important items of fact are never mentioned at all. Space does not permit detailed documentation, but those familiar with the literature will recognize many omissions and distortions.

The book should, however, become a standard source for those interested in studying this country's official ideology on the narcotics question. Some of the important points in this ideology are: The addict tends to be a "criminal type" with "psychopathic tendencies" who takes the drug primarily because it panders to some abnormal psychological need. He can be cured of his physical addiction through enforced withdrawal from drugs and careful follow-up treatment; the mental aspect of addiction can, in many cases, never be overcome. Stronger penalties are needed for addicts and peddlers, up to and including life sentences for those incapable of abandoning their vicious ways. Under no circumstances must an addict be allowed drugs to alleviate withdrawal symptoms, for this would be temporizing with evil.

There is some reason to believe that this ideology, moralistic to the point of ignoring facts, constitutes in itself one of the causes of our present narcotics problem. As a full and detailed expression of this viewpont, the book deserves the careful attention of all students of the phenomenon.—HOWARD S. BECKER

Health in the Later Years (Institute of Gerontology Series, Volume 3). Edited by JOHN M. MACLACHLAN. Gainesville: University of Florida Press, 1953. x, 123 pp. $1.50.

The third in the Annual Southern Conference on Gerontology series, the theme of this book is "Florida Plans for a Healthy Senior Citizen." Representatives of various specialties bearing on problems of aging discuss the health of the aged from differing viewpoints. Both Drs. E. L. Burtz and F. J. Sladen, agreeing that medical interests in "geriatrics" is negligible, call for a "gerontological" approach covering a wider age span. E. W. Burgess, R. M. Ball, R. W. Kleemeier, A. I. Lansing present ways in which sociology, economics, psychology, and biology can contribute to understanding the older person.

N. W. Shock calls for gerontological research in the University of Florida's new medical school, while H. E. Handley adds that the University's responsibility extends to coordinating educational and research programs with those of the community services. H. S. Smith points out the need for subsidization of medical care for the indigent aged. S. D. Doff and F. M. Hall speak of the need for coordinating the autonomous medical services for the aged through the local and the State Board of Health. On medical education, R. A. Moore and W. T. Sanger point out the need for a wider understanding of the whole life-process to appreciate problems of aging.

An interesting adjunct to Mr. Smith's paper is a group of 22 responses to the question, "Has there been a particular stimulus toward adequate provisions for the medically indigent aged person (diagnosis, care, and prevention) because of the medical schools in your state?" from representatives of medical schools, medical societies, public welfare and health program administrators in eight Southern states. In general, the answer is negative.—BERNARD KUTNER

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